

Hooker (W.)

PRIZE ESSAY.

RATIONAL THERAPEUTICS;

OR,

THE COMPARATIVE VALUE OF DIFFERENT CURATIVE MEANS, AND
THE PRINCIPLES OF THEIR APPLICATION.

"NATURA DUCE."

BY WORTHINGTON HOOKER, M.D.

OF NEW HAVEN.

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Extracts from the Records

OF

THE MASSACHUSETTS MEDICAL SOCIETY.

At a Meeting of the Councillors, Oct. 3, 1856, —

The Treasurer announced, that, through the liberality of one of its Fellows, the Massachusetts Medical Society is authorized to offer the sum of One Hundred Dollars to the author of a Dissertation which may be adjudged worthy of a prize by a Committee appointed by the Councillors of the Society, on the following subject, viz.: "We would regard every approach towards the rational and successful prevention and management of disease, without the necessity of drugs, to be an advance in favor of humanity and scientific medicine."

At a meeting of the Councillors, Oct. 1, 1856, the following gentlemen were appointed the Prize Committee, viz.: —

Dr. AUGUSTUS A. GOULD.
Dr. CHARLES G. PUTNAM.

Dr. FRANCIS MINOT.
Dr. HENRY W. WILLIAMS.

Dr. ANSON HOOKER, Senr.

At the Annual Meeting of the Society, June 3, 1857, —

Dr. GOULD, Chairman of the Prize Committee, reported that six Essays had been received; that the Committee had unanimously agreed upon the Dissertation considered worthy of the prize; and handed to the President the envelope bearing the motto of the Dissertation. On breaking the seal, the author was found to be Dr. WORTHINGTON HOOKER, of New Haven, Conn.

DISSERTATION.

"WE WOULD REGARD EVERY APPROACH TOWARDS THE RATIONAL AND SUCCESSFUL PREVENTION AND MANAGEMENT OF DISEASE, WITHOUT THE NECESSITY OF DRUGS, TO BE AN ADVANCE IN FAVOR OF HUMANITY AND SCIENTIFIC MEDICINE."

THIS proposition¹ embodies a grand lesson, which the medical profession has been learning, especially during the past half-century ; and it is yet far from having come to the end of the lesson. The recorded medical experience of this period, which has been so much more busy than any other period in true rational observation, exhibits, in a great variety of ways, a marked tendency to a diminution of active medication. This tendency was but feebly and fitfully manifested during the first portion of this period, and was, for the most part, overborne by a contrary tendency : but its struggles became more and more strong and steady ; and, for the past quarter of a century, it has been predominant in the profession. And the result is seen, at the present time, in the prevailing disposition to exalt negative means of cure, above those which are positive ; means that quiet, above those that disturb ; and simple means, above those whose *modus operandi* is occult, and is the subject of theory and discussion. Drugs, especially those which depress and disturb, have had greater and greater limitations put upon their

¹ It is proper to state, that this proposition is an extract from the excellent Address of Dr. A. A. GOULD, delivered before the Massachusetts Medical Society at its annual meeting in 1855, p. 43.

application ; while other remedial means, not included under this term, have more and more engaged the attention of practitioners.

Allied to this tendency, and somewhat involved with it, is another, which is perhaps to have a still greater influence upon the sanitary welfare of the community. I refer to the disposition, which has so decidedly increased during the last twenty-five years in the profession, to seek out the causes of disease, and to guard against their action. Very appropriately, therefore, is the prevention of disease coupled with its treatment in the proposition which is the subject of this essay. And yet a full consideration of this portion of the proposition would mar the unity of the plan which I have marked out for myself ; and would, besides, require more space than can properly be given to it. I shall therefore bestow upon it here but a brief notice ; and shall afterwards allude to it only incidentally, as its connection with the other part of the proposition shall require.

Although much has been said of late years, by physicians and others, on the subject of hygiene, the community generally manifest but little interest in the prevention of disease, while they have an eager and absorbing interest in whatever relates to its cure. This is seen to be a glaring inconsistency, when we consider that the chief causes of disease are more or less under our control, some of them entirely so ; and the ravages of those which are beyond our control, as the contagions, and some of the causes of epidemics, can be very much lessened by guarding against the action of the common causes of disease. It is often the co-operation of the latter that gives virulency to the former, and occasions their wide diffusion.

The notion which has been entertained by some ultraists, that there would be almost no disease if the laws of life were properly regarded, is extravagant and wild. With all the control that it is possible for us to exercise over the circumstances that surround us, the incidents of our condition must

render us, to a considerable extent, liable to disease. And yet that disease may be in a great degree diminished by preventive measures, is not for a moment to be questioned: indeed, there is not a doubt that it can be diminished vastly more by these than by curative measures.

If it were necessary, statistics might be given bearing upon this point. The opinions also of physicians, whose attention has been particularly drawn to this subject, might be cited. I will content myself with citing but one opinion, as a specimen, — that of the venerable Dr. James Jackson, of Boston, who is always careful and deliberate in his statements. The question put to him was this: “How great a proportion of disease, of suffering, of diminution of physical capacity, of usefulness, and of abridgment of life, comes from sheer ignorance; and which, therefore, we might hope to see averted, if the community had that degree of knowledge which is attainable by all?” His reply was, “I feel assured that the answer should be, *More than one-half.*” When it is brought to mind that the ignorance of parents is included in the inquiry, the justice of the answer will probably be admitted by all who are conversant with the subject.

What has been said of prevention is especially true of chronic diseases. Phthisis, that most common and most destructive of all the maladies of this class, is a striking example. The causes which predispose to it are such, that they can, to a great extent, be either avoided or neutralized. Even the hereditary influence, which acts so largely in the production of this disease, is far from being inevitable in its results. I will not go into an extended notice of this important point, but will barely mention one of the many facts that demonstrate it. In families that have in successive generations shown a disposition to phthisis, it is common to see the female members succumb to it; while the habits of the male members, tending to make them more robust, enable them to resist it. And the difference in this respect

is always lessened when the habits of the male members of the family are sedentary. All the facts which have been gathered in relation to the causes of this disease show that quite a large proportion of its victims might be saved by preventive means; while curative means succeed in redeeming comparatively few of them.

It is proper to remark here, that many of the measures which tend to prevent the occurrence of some chronic maladies are at the same time the principal means of their cure. This is true of phthisis. Those means which give vigor to the system are alike preventive and curative; their use being, of course, modified by the varying circumstances of the individual cases. Chronic diseases of a nervous character, and those which are compounds of local affections and a generally debilitated and irritable condition, are cured chiefly by a modified use of the same measures that fortify the system against such diseases. These are measures which, however, do not commonly get the credit which they deserve from the community; nor even, in all cases, from medical men. The error committed by John Wesley, who was, as Dr. Paris says, "more disposed to attribute his cure to a brown-paper plaster of egg and brimstone than to Dr. Fothergill's salutary prescription of country air, rest, asses' milk, and horse-exercise," has been largely repeated, even up to the present time; and all quacks, and many even who are in the ranks of the profession, are glad to have this error perpetuated.

What I have said of chronic diseases is true, to a limited extent, of many of the forms of acute disease. Some of the curative measures are among the most important of the preventive measures,—as the free access of pure air, and the exclusion of all sources of excitement and other deleterious agents.

To some extent, then, the prevention of disease coincides with its management; but, for the reasons before stated,

I shall chiefly confine my attention in this essay to the latter.

My plan will be to illustrate the proposition under consideration from the history of medicine, especially during the last half-century; drawing such lessons from the points brought out, that I may, in conclusion, indicate certain principles for the guidance of practitioners in the investigation of therapeutics, which may secure the full results contemplated by the proposition, and thus place therapeutics upon a more rational basis than it has as yet attained.

It will be seen that my object is not merely to demonstrate the truth of the proposition. This would be of comparatively little benefit, and might allow of error in a too extensive and indiscriminate abandonment of positive medication. I propose to go beyond this general view of the subject, and gather up those facts in the past experience of the profession which may be of use in discovering the limitations which should be put upon the application of remedies. I shall also endeavor to develop the principles which should guide us in fixing upon these limitations. The tendency of such an investigation will be, not merely to narrow the limits of active medication, but to render it much more definite in its aims, and even to widen the actual range of its efficiency. It is the true way to relieve medicine, so far as it can be done, of the uncertainty which is attached to it.

Up to the latter part of the last century, medication was for the most part of little efficacy. The *Materia Medica* was indeed extensive; but most of the remedies were of an inert character. Some of the medicines in common use were made up of these remedies compounded together, sometimes to the number of twenty, fifty, or even a hundred; and, though many of them were even more inert than our modern sarsaparilla, great efficacy was attributed to them, the notions in relation to their mode of action being vague and fanciful. It was from the prevalence of notions of this character that such remedies as frogs' spawn, powder of crabs'

claws, the flesh of vipers, dried toads, &c., were introduced into the multifarious compounds then in vogue.¹ These compounds were generally composed of ingredients so various in their character, that it would be impossible to divine the purposes which they were to accomplish. The noted Theriac of Andromachus, or *Antidotum Mithridatum*, though the number of its ingredients varied somewhat from time to time, had in the Codex Medicamentarius of Paris, under the appropriate grandiloquent name of *Electuarium opiatum polypharmacum*, seventy-two ingredients, thus: Acrid substances, 5; astringent, 5; bitter, 22; indigenous aromatics, 10; umbelliferous aromatics, 7; balsamic and resinous, 8; fetid, 6; narcotic, 1; earthy, 1; gummy or amylaceous, 4; saccharine, 3. One of these ingredients was the flesh of vipers. The narcotic substance was opium; which certainly would do better without such an array of auxiliaries, some of them of quite a questionable character.

That all the medication of those times was marked by such polypharmacy, I do not assert. But although there is some evidence occasionally of a disposition in such minds as Sydenham and Boerhaave to discard it, and adopt a more simple mode of therapeutics, yet they were far from breaking away fully from the prevalent custom, and farther still from making any systematic attempt to rid the *Materia Medica* of its useless and even disgusting rubbish. Some medicines of a decided character, such as cathartics, emetics, and opiates, were, it is true, used, either alone or in connection with the

¹ Some of the enemies of the medical profession have cast reproach upon it, on account of its use of inert and disgusting remedies in olden times. But it is to be recollected that such follies were far from being confined to medical men in those days. Wise men of all classes had mingled with their wisdom most strange and ridiculous notions. And it is also to be remarked, that many of the past errors of the medical profession are now perpetuated in the doctrines and practices of quackery. Thus that masterpiece of quackery, Homœopathy, has such inert remedies as oyster-shell, silica, pulsatilla, &c., to which it attributes most marvellous effects; and, as to disgusting remedies, it surpasses all that can be found in the darkest times of the medical profession in its infusion of the *pediculus capitis*, which Dr. Mure, styled in England "the apostle of Homœopathy," proclaims as the grand remedy for most chronic diseases.

inert articles in such common use ; but they were used sparingly. So also, from the time of Paracelsus and the chemists, mercury and antimony were occasionally used ; but their effects were little understood, and the general prejudice against them was very strong. So strong was the prejudice in France against antimony, that this drug was publicly condemned, and was a long time in gaining the popular favor.

No one exerted so much influence as Cullen in disencumbering therapeutics of its mass of useless materials, and in introducing definite ideas of the action of remedies upon disease. In the preface to his "Practice of Physic," he makes some remarks on previous systems of practice, which show what the state of things was when he came upon the stage. I cannot forbear making an extract. In speaking of the influence of the Stahlian system upon the practice of physicians, he says, "Trusting much to the constant attention and wisdom of nature, they (Stahl and his followers) have proposed the *art of curing by expectation* ; have therefore, for the most part, proposed only very inert and frivolous remedies ; have zealously opposed the use of some of the most efficacious, such as opium and the Peruvian bark ; and are extremely reserved in the use of general remedies, such as bleeding, vomiting, &c."

"Although these remarks," he goes on to say, "upon a system which may now be considered as exploded or neglected, may seem superfluous, I have been willing to give these strictures on the Stahlian system, that I might carry my remarks a little farther, and take this opportunity of observing, that, in whatever manner we may explain what have been called the operations of nature, it appears to me that the general doctrine of *Nature curing diseases* — the so much-vaunted *Hippocratic* method of curing — has often had a baneful influence on the practice of physic, as either leading physicians into, or continuing them in, a weak and feeble practice, and, at the same time, superseding or discour-

aging all the attempts of art. Dr. Huxham has properly observed, that, even in the hands of Sydenham, it had this effect. Although it may sometimes avoid the mischiefs of bold and rash practitioners, yet it certainly produces that caution and timidity which have ever opposed the introduction of new and efficacious remedies. The opposition to chemical medicines in the sixteenth and seventeenth centuries, and the noted condemnation of antimony by the medical faculty of Paris, are to be attributed chiefly to those prejudices, which the physicians of France did not entirely get the better of for near a hundred years after. We may take notice of the reserve it produced in Boerhaave, with respect to the use of the Peruvian bark. We have had lately published, under the title ‘*Constitutiones Epidemicæ*,’ notes of the particular practice of the late Baron van Swieten; upon which the editor very properly observes, that the use of the bark in intermitting fevers appears very rarely in that practice; and we know very well where Van Swieten learned that reserve.”

During the eighteenth century, the influence of those leading minds — Sydenham, Stahl, Hoffman, and Boerhaave — was largely but variously manifested in medical practice. The influence of Boerhaave was continued through Van Swieten, his illustrious pupil, and other prominent physicians, his admirers. Therefore, when Cullen began the study of medicine, he says that he “learned only the system of Boerhaave;” and, when he became a professor in the University, he found this system there in full force. Meanwhile, in France, Lieutaud was the great medical man of the times; of whom Cullen says that he is “very much upon the old plan of following nature, and therefore gives often what I consider as a feeble and inert practice. The *humectantia*, *diluentia*, *demulcentia*, *et temperantia*, are with him very universal remedies, and often those which are alone to be employed.”

There was, during the last century, some advance made in

positive medication ; but it was a hesitating and vacillating movement. It was reserved for Cullen to usher in the new era, for which preparation had thus been made by those who immediately preceded him. He was born in the first part of the last century, in 1712 ; but his full influence was not felt till its close : for, although he was a long time a teacher of medicine, it was not till 1772 that he published his "*Materia Medica* ;" and his "*Practice of Physic*" he published in 1784, only six years before his death.

The writings of Cullen gave to medical practice everywhere a more definite and decided character. Especially did such remedies as calomel and antimony come into more common use. Dr. James Hamilton, of the University of Edinburgh, in a very judicious work on the use and abuse of mercurial medicines, published in 1820, says, that, "for some ages after mercury became an article of the *Materia Medica*, physicians recommended it only on the most urgent occasions ; but, within these few years, British practitioners seem to have overlooked the necessity for such caution, and to exhibit that medicine with very little scruple." He also says, that "calomel is now in Great Britain almost the universal opening medicine recommended for infants and children ; and a course of the blue pill (which is one of the mildest preparations of mercury) is advised, without any discrimination, for the cure of trifling irregularities of digestion in grown persons." And the same may be said of its use in this country at that time.

Other remedies of power were used with the same freedom, such as bleeding, emetics, purgatives, opiates, &c. An active medication was generally introduced. "The art of curing by expectation," so decidedly denounced by Cullen, was in no favor. Disease was to be attacked ; it was to be overcome ; it was to be broken up. This was the language of the times. The recuperative energies of nature were to be trusted but seldom and sparingly. Most practitioners followed the lead of Cullen, who held this language on the subject : "Although

this *vis medicatrix naturæ* must unavoidably be received as a fact, yet, wherever it is admitted, it throws an obscurity upon our system ; and it is only where the importance of our art is very manifest and considerable that we ought to admit of it in practice." Some went even farther than this. This was true of Dr. Rush, who had such wide and long-continued influence upon American practice. One of the most distinguished physicians in this country recollects hearing him use, in the lecture-room, this strong language in relation to nature's curative efforts : "As to nature, I would treat it in the sick-chamber as I would a squalling cat, — open the door, and drive it out."

The reign of active medication, thus established chiefly by Cullen, reached its culminating point somewhere in the first quarter of this century. It was not introduced by him in full, but was fairly begun, and then was consummated in the course of a few years by those who followed him. This may be very distinctly seen in relation to the use of mercury. Cullen, it must be evident to every one that reads his "Practice of Physic," had no idea of the extent to which this remedy was destined to be applied by his successors in the treatment of inflammations and fevers, much less of the common and indiscriminate use of it described by Hamilton.

During the past twenty-five or thirty years, the reign of active medication has been manifestly declining. This decline is to be attributed mostly to the diffusion of more discriminating views in the profession in relation to the operation of remedies. Some, however, are inclined to consider it as owing very much to a change which they suppose to have taken place in the general character of diseases. But the reality of this change is questioned by many who are quite as reliable observers as those who assert that it has occurred ; and their view of the question certainly has some show of reason, if we consider the agencies which are necessary to the production of so great a change as has been claimed to have taken place. For observe, that it is not a

change in the character of any particular diseases, and from one season to another; but it is a change covering a long term of years, and in the general diathesis of disease. This, it is asserted, is much more disposed to be asthenic than it was in the days of active medication. The fact, so distinctly observed by Sydenham, that epidemic diseases thus change from year to year, from modification of some of the concurrent causes that produce them, has been well established by the observation of physicians since his time. But the change under consideration is altogether different from this: it is a change which could not be produced, unless a continuous influence of some considerable power were exerted during a series of years, alike in healthy and unhealthy seasons, when epidemics were rife, and when they were mild, or even absent, modifying the action of the common causes of disease, so as to alter its general character in all its forms. And, besides, to effect so great a change as has been asserted, so abruptly and so thoroughly, some alteration in the very character of the human system would seem to be required.

It appears clear, then, from these considerations, in addition to the fact that the reality of the change is matter of dispute, that there cannot have been an alteration in the general diathesis of diseases, sufficient to account for the great and general change in medical practice that we have witnessed in the past twenty-five or thirty years.

During the reign of active medication, there were great strifes in the profession. The most opposite modes of practice were advocated in the treatment of the same disease, and physicians were often divided into fiercely opposing parties. I will refer to but a single example. In the first quarter of this century, there was in New England a great contest between two rival parties, in relation to the general character of diseases and their treatment. Dr. Gallup and his followers maintained that diseases were almost wholly sthenic or inflammatory, and therefore depleting remedies were called for; and, of these, bleeding was by far the most

important. Drs. Miner and Tully, on the other hand, contended that the asthenic disposition predominated; and their remedies were of an opposite character to those of Dr. Gallup. They made large use of opium and stimulants. They also used calomel in connection with these. The language which these two rival schools held towards each other was of the most decided character. Thus Dr. Gallup says, that "it is probable, that, for forty years past, opium and its preparations have done seven times the injury they have rendered benefit on the great scale of the world." And Dr. Tully says of Dr. Gallup's mode of practice, "The lancet is a minute instrument of mighty mischief, — a weapon which annually slays more than the sword. Antimony alone does more injury than all the efficient exciting and supporting agents of the *Materia Medica*." And again: "The King of Great Britain loses every year more subjects by these means (that is, depleting means) than the battle and campaign of Waterloo cost him, with all their glories." Dr. Miner says of the same means, that they "have been the scourge and devastation of the human race for more than two thousand years."

In this, as well as in all other cases in which such opposite views of treatment have been held, the appeal to results was made equally by both the opposing parties; and, from the statements which were made, it would be impossible to decide which practice was the most successful, or, rather, which was the least unsuccessful.

In view of such opposing testimonies in relation to modes of treatment, some have been inclined to the conclusion, that medicine is of no avail, and that the sick had better be given up to the recuperative efforts of nature, the physician only so managing the diet and regimen as to favor these efforts. But, while the facts do not forbid such a conclusion, they by no means fairly lead to it. It is indeed a legitimate conclusion, that the cases treated under the opposing modes would have done better with no medication; that is, the gross

results would have been better. But the facts are far from proving that the absence of all medication would have been followed by better results than a judicious application of general principles, — the measures of both modes being adopted to some extent, and adjusted to the needs of individual cases. They do not touch this point at all. This must be decided by altogether different data.

It will not be deemed unprofitable here to consider briefly the question, whether positive medication is of real benefit in the treatment of disease generally. The question, let it be borne in mind, is, not whether the results of medical practice, as pursued by the profession at large, are better than they would be if disease were left to nature and a proper regimen; but whether this is true of the ordinary practice of judicious physicians. That it is so, is proved by various kinds of evidence, which I will very briefly notice.

There is very decisive evidence on this point in the actual, undoubted effects of remedies. It is often difficult to distinguish between the favorable effects of medicines and the results of nature's efforts; but there are cases in which there cannot be the slightest doubt as to the curative influence of the remedies that we employ. The evidence from such cases is clear on the simple question, whether medication is ever useful. To what extent it is applicable is quite another question, to be decided by other evidence.

We have evidence also from comparisons between cases subjected to medication and those which are left to nature. This evidence, however, is limited in amount; because the belief in the propriety of using some positive means of treatment in disease is so universal, that such comparisons are very seldom made. One of the most conclusive which we have is that reported by M. Grisolle in relation to pneumonia. In 1840, he treated eleven patients attacked with this disease, without using any active remedies. The cases which he selected for observation were such, that there was no reason to fear a fatal termination in any of them; and

yet they were sufficiently marked to test the question. The patients were kept quiet in bed, with a light diet ; and the only medicine given to any of them was some mild laxative to obviate constipation. The result was, that the prominent symptoms of the disease continued much longer than they did in other similar cases which were treated in the usual way. The pain in the side, in some of the cases, continued so long, that M. Grisolle felt called upon at length to use cupping and blistering for its removal. This experiment or observation does not, it is true, touch the question of rate of mortality directly : but it does indirectly ; for it is proper to infer, that remedies which relieve and shorten disease when it is mild, will tend to save from a fatal result when the disease is severe.

But we have some evidence furnished to us unwittingly by Homœopathy on quite an extended scale. I refer to the famous returns of the hospital of Fleischmann, near Vienna. These returns were offered to the public as decisive proofs of the superiority of the Homœopathic practice over the common practice of physicians. If the gross results alone had been given by Fleischmann, it would have added another to the many errors that have been palmed upon the world by bare statistics ; but, fortunately for the truth, though not so for his interest and for that of Homœopathy, he entered into particulars in relation to the number of cases of different forms of disease, so that we are enabled to make something like a fair comparison of the results with those of medical treatment in other hospitals. And such a comparison leads us inevitably to the conclusion, that the treatment of Fleischmann was much less successful than that which is followed in hospitals under the care of regular physicians. Now, as he is a true Homœopathist, and adheres to the infinitesimal doses of Hahnemann, we have here a comparison between the ordinary practice of hospitals and a practice which in reality leaves every thing to nature, regulating only the circumstances and the diet of the patients so as to favor nature's salutary operations.

I will notice these returns of Fleischmann very briefly, in order to show the grounds of this conclusion. His returns cover 6,501 cases treated in his hospital during a period of eight years. The mortality, which was 6.4 per cent, is claimed by him to be a small percentage of mortality in comparison with hospitals managed under the common practice. While this is true of some hospitals, it is not true of others. Some have a mortality considerably below that of Fleischmann's hospital. For example: Mr. Thomson states the mortality of the Dundee hospital to be 5.1 per cent; that of Aberdeen, 4.6 per cent; that of Inverness, 4.3 per cent; and that of thirty provincial hospitals, taken together, 4.4 per cent. The average mortality of the English military hospitals is only 2 per cent. Those hospitals which have a mortality above that of Fleischmann's are in the midst of large cities; for they are crowded with patients, and from this cause often reject applications for the admission of mild cases. But although Fleischmann's hospital was situated out of Vienna, in the suburbs, and was therefore not liable to be crowded with bad cases, his mortality is only a little less than that of the largest hospitals in London, as observed during four years; this being 8.4 per cent, while his was 6.4 per cent.

And this is not all. On examining Fleischmann's report in relation to the diseases of the inmates of his hospital, it is found that there is an uncommonly large number of cases of mild and curable diseases; while the number of cases of severe and incurable diseases is comparatively small. Dr. Simpson, from whose book on Homœopathy I glean these facts, has drawn a comparison in this respect between Fleischmann's hospital and the Edinburgh hospital, which in two years admitted nearly the same number of patients that Fleischmann's did in eight years. This comparison I will give as concisely as possible.

From an examination of a large number of hospital reports, Mr. Thomson, as quoted by Dr. Simpson, found that the percentage of mortality depends chiefly upon the number of

cases admitted of the following diseases: 1. Pulmonary consumption; 2. Organic disease of the heart; 3. Aneurism of the large vessels; 4. Organic disease of the kidneys; 5. Organic disease of the stomach. Of these five forms of disease, there were only 120 cases admitted into Fleischmann's hospital; while 548 were admitted into the hospital at Edinburgh. And, if other forms of disease which are apt to end fatally be taken into the account, the difference between the two hospitals will be seen to be still greater than this, as is represented in the following table:—

	No. of cases in Fleischmann's Hospital.	No. of cases in Edinburgh Hospital.
Consumption	98	276
Palsies	5	103
Organic disease of the heart . . .	15	159
Organic disease of the liver . . .	1	33
Bright's disease of the kidney . .	0	82
Diabetes Mellitus	0	17
Internal Aneurisms	1	18
Caries and Necrosis	5	57
Malignant (cancerous) tumors . .	0	55
Other tumors	0	36
	<hr/> 125	<hr/> 836

Besides all this, the difference between the two hospitals, in regard to cases of severe external injuries, is very great. In Fleischmann's hospital, there were only 52 cases of injury; and, of these, 34 were slight wounds, all of which recovered; and 18 were cases of burns, of which two died. But, in the Edinburgh hospital, there were 641 cases of injury; and 150 of them were subjects of the "principal operations," of which 32 died.

Again: all cases admitted when moribund are inserted in the reports of the Edinburgh hospital; while Fleischmann excludes them. This materially affects the percentage of mortality.

Dr. Simpson makes a comparison also between the two hospitals, in relation to mild and curable cases of disease,

thus : “ While, among the 6,000 Edinburgh-hospital cases, we have 34 cases of that non-fatal disease, inflamed sore-throat, or *cynanche tonsillaris*, among the 6,000 Vienna Homœopathic cases, there are no less than 301 cases of this affection. In the Edinburgh returns there are two cases, and in the Vienna returns 110 cases, of chicken-pox ; in Edinburgh one case of herpes or tetter, in Vienna 20 cases ; 48 cases of chlorosis and amenorrhœa at Edinburgh, and 90 at Vienna ; 37 cases of headache at Edinburgh, 61 at Vienna ; 52 cases of influenza at Vienna, and none at Edinburgh ; and so on.

So unusual a difference as this between these two hospitals, in regard to the character of their cases, is not to be referred merely to the fact that one was in the midst of a city, and the other was in the country. In Fleischmann’s hospital, there was undoubtedly much management in courting the admission of favorable cases, and in getting rid of those which were manifestly incurable ; and, with his large proportion of mild and curable cases thus obtained, the percentage of mortality is far from being in favor of his treatment. Assuming, as we have undoubtedly a right to do, that he gave his patients infinitesimal doses, we may say that this comparison between the two hospitals shows, most conclusively, that leaving disease entirely to nature is much less successful than a judicious positive medication. If this were not so, Fleischmann ought to have made out a much smaller percentage of mortality under all the favoring circumstances which have been mentioned. If any hospital under the control of “ regular medicine ” could be managed under similar circumstances, and with the same manœuvring in the admission, exclusion, and discharge of cases, vastly better results would be realized than those which Fleischmann has so vauntingly spread before the world. Our hospitals actually do much better than his did, even with much less favorable circumstances ; as, for example, the provincial hospitals, whose average mortality is 4.4 per cent ;

while that of Fleischmann's is 6.4 per cent, — about one-third more. The mortality of the English military hospitals is even less than that of the provincial hospitals, — only two per cent.¹

Having thus definitely settled the question, whether active medication is of any value, we come to another inquiry of a much more complex character; viz., Of how much value is it? or, in other words, What are the limits of its applicability? And another inquiry also is naturally coupled with this; viz., What are the principles which should guide us in fixing on the due limitations of positive medication in individual cases of disease?

These inquiries are not only complex, but difficult; and hence they are not fairly met by the great body of the profession. The rigid investigation which is requisite for this is unwelcome to those who are fond of the easy path of theoretical practice, or the easier one of routine; and the principles which should guide such an investigation have really been but imperfectly developed, and can for the most part only be gleaned here and there from the standard works of the profession.

I shall endeavor, in the following pages, to evolve from the experience of medical men answers to the inquiries which I have stated. For this purpose, I shall examine some of the

¹ Dr. Gairdner, as quoted by Dr. Simpson, remarks of Fleischmann's hospital, "If I were to give a formula for the management of a hospital designed to exhibit a low rate of mortality, it would be this: Choose your site well; let it be not *in* but *near* a large city, having already hospital accommodation on a prodigious scale, well known to the poorest classes of the community, and adapted to their wants; let the distance from the centre be such (say three miles) as will keep back the extremely abject and the dangerously diseased, either through want of knowledge of your institution, or want of power to reach it; let the arrangements be so perfect as to contrast favorably with the older hospitals, and to attract the valetudinarians, whose illnesses and means permit them to avail themselves of its superior accommodation; and, finally, let some special practice be pursued, in order to enlist the sympathies of rich or idle *dilettanti*, who will know how to fill your wards with the sort of cases suitable for your experiment. THIS IS PRECISELY THE PICTURE OF THE VIENNA HOMŒOPATHIC HOSPITAL, which has the amazing effrontery to call on us to compare its peddling *experiments* with the great labors of pure beneficence, of which general hospitals of this and other countries furnish examples."

changes that have been made in medical practice during the last twenty-five or thirty years, and, from the limitations and modifications already made in therapeutics, deduce those principles which will enable us to make still farther limitations and modifications. I do this with the firm belief, that, by such an investigation, as before hinted, we shall come at the principles on which therapeutics may be made much more definite than it now is, and practical medicine be relieved of much of the uncertainty with which it is supposed to be unavoidably enveloped.

I have said that the reign of active medication was at its height in the first quarter of this century ; but it was by no means an undisputed reign. Many physicians then pursued a less active treatment than the mass of the profession, and some publicly protested against the dominant error. For example : Dr. Falconer, of Bath, as early as 1809, in a paper in the first volume of the Transactions of the Medical Society of London, in strong language pointed out the dangerous effects of the prevalent indiscriminate use of mercury. But Dr. Hamilton says, about ten years after, that his warning voice was not regarded, and that "the employment of mercurial medicines has for several years become more and more extensive."

So, on the other hand, when the reign of active medication really began to decline, there was much opposition to the movement in the profession. Hence it is that Dr. Bigelow, in his valuable paper on Self-limited Diseases, read before the Massachusetts Medical Society in 1835, makes use of such language as this : "In many places, at the present day, a charm is popularly attached to what is called an active, bold, or heroic practice ; and a corresponding reproach awaits the opposite course, which is cautious, palliative, and expectant." But multitudes of observers had adopted more or less the same views that Dr. Bigelow developed so clearly in his paper ; and the decline of positive medication became more and more general in the profession. The changes which

have taken place in this movement I propose now to notice, first in relation to certain remedies, and then in relation to diseases.

The change which has been made in the use of mercury, during the past twenty-five or thirty years, presents many points of interest. Its use for some purposes has been entirely discontinued. It was quite largely used for some length of time in fever, with the idea, that, if the constitution could be brought under its influence, the fever would be removed; a mercurial fever, as it might be called, taking its place, which the recuperative powers of nature could easily remove. Although in some cases this mode of practice seemed to produce the effect intended, it was found, on the whole, so frequently to fail, and to be attended sometimes with such disastrous results, that it has been wholly abandoned. Mercury is used, at the present time, very sparingly in fever, and only so far as it is needed to affect the secretions, or to combat accompanying inflammation.

Mercury is also discontinued from use in the exanthematous diseases, unless there be some special reasons in the complications of these diseases for its employment. At one time, its use was highly lauded by Dr. Armstrong and others as a remedy in scarlatina; but, at the present time, there is no truth more definitely settled by the experience of the profession, than the impropriety of its use as a common remedy in this malady.

Mercury is no longer used as a common cathartic. It has come to be the settled practice of the profession to avoid its use, in this respect, in all cases where the distinctive effects of this drug are not called for. This is in strong contrast with the incautious and indiscriminate use of this remedy which was so prevalent, both in and out of the profession, during the first quarter of this century.

The same discriminating experience which has discarded mercury as a general remedy in fever and in the exanthemata has retained it in the treatment of inflammations; but it

has been found that it need not be pushed to the extent that was formerly supposed to be necessary in this class of diseases. In most cases, it is not necessary even to affect the gums; and salivation is always to be avoided; although, in some severe cases, it is proper to run the risk of producing it.¹

The combination of calomel, antimony, and opium, which, in various proportions, is now so much used, is a remedy of very great value in the treatment of inflammatory diseases. To Dr. Robert² Hamilton, of Lynn Regis, England, the credit is commonly given of first drawing the attention of the profession, in the year 1783, to the efficacy of this combination. He says that its usefulness was first suggested to him in following out a hint given to him by an army surgeon, in relation to the use of calomel in the treatment of acute hepatitis. He added opium to the calomel to relieve the pain attendant upon inflammation, and then antimony to relieve the febrile excitement and to produce perspiration; and, inferring that this combination might be serviceable in the treatment of the inflammation of other organs as well as

¹ A valuable paper has been published recently by Dr. Henry W. Williams, of Boston, showing that iritis can be treated successfully without the use of mercury and active antiphlogistic measures. If his results shall be verified by others (and I see not why they should not be), Dr. Williams will produce as great a change in the practice of the profession in this disease as Dr. Ware has in its practice in delirium tremens. Farther investigations are needed, not so much to confirm the main point in his paper, as to determine how much the different remedies which he used had to do with effecting the cure. I apprehend that it will be found that the opium given internally, and the belladonna used locally, were the effective remedies; while the quinine and the iodide of potassium are of small account. It is to be regretted that Dr. Williams was not more particular in reporting the amounts of opium that were taken by the different patients.

² Too much has been claimed for Dr. Hamilton, by his countrymen, in attributing to him the first introduction of calomel in the treatment of inflammatory maladies. To an American physician — Dr. Douglass, of Boston — belongs the credit of this. He introduced it as early as 1736; and, by the middle of the century, it was quite the common practice among American physicians in pneumonia, pleurisy, rheumatism, &c. Dr. Hamilton's observations were made many years after this; and to him belongs the credit of adding, to what had been established by Douglass and the American physicians generally, what he discovered in regard to the combination of other important remedies with the calomel, as stated in the text.

that of the liver, he proceeded to verify the inference, and thus inaugurated a practice which has become established by abundant experience as one of the permanent advances of the profession in the treatment of a very wide range of diseases.

Mercury is a remedy of great value in the treatment of many chronic diseases ; but, during the reign of active medication, it was used in them too largely, and often with so little discrimination, that disastrous results were produced. Not only is it now used much more cautiously in these maladies, but, in very many cases in which it would formerly have been deemed applicable, it is now given up, as calculated only to add to the sufferings of the patient, without effecting any good result, or to leave him in a bad condition after the cure of the particular disease for which it is used is accomplished, or even to prevent a cure which might have been effected if more gentle means had been employed. In Dr. James Hamilton's book on the "Use and Abuse of Mercerial Medicines," there are many interesting facts stated bearing upon these points.

Emphatically may it be said, in view of the sad results which have come from the needless use of mercury, that the great diminution of its use in the treatment of disease is "an advance in favor of humanity and scientific medicine." At the same time, it is to be borne in mind, that it is far from being a mere general diminution. It is a diminution which is based upon an extensive range of discriminations ; so that, while in some cases where it was formerly used it is now wholly discarded, and in others it is used with much less freedom, there are some cases in which its introduction into the system is effected as rapidly as possible ; and cases occasionally occur in which there is some reason to think that it is proper to use it in exceedingly large doses. This last point, however, is as yet *sub judice*.

Some of the most valuable acquisitions which the profession has made in therapeutics during the present century are

the discriminating limitations that it has been able to put upon the use of this remedy, which is one of the most efficient of its active means of cure. The advance which has been effected in this respect step by step in the profession's experience is greater than is ordinarily supposed. Larger additions have in this way been made to our real means of cure than by any, or even perhaps all, of the new remedies that have been discovered during the same period of time.

All disturbing remedies are much less in vogue now than they were in the first quarter of this century. Physicians then very commonly used such remedies, especially in the beginning of attacks of disease, for the purpose of breaking up the attack, or of lessening its force. Emetics were common remedies for this object. The practice was applicable in some cases; but it was far too generally employed. So common was the plan of thus adding to the turmoil of disease at the outset, that it was a popular saying, that it was necessary to make one worse in order to make him better. And this disturbing and depressing mode of treatment was by no means confined to the beginning of disease; but it was customary to continue it to some degree during the progress of the case. Febrile symptoms, so long as they lasted, were combated actively; and active remedies were always addressed to the removal of any local derangements that might exist, little dependence being placed upon the recuperative efforts of nature. But now the general character of medical practice is vastly different in the points alluded to. In comparatively few cases, even of acute disease, is the patient made worse at the beginning, in order to make him better. Generally he is made better at once, by measures that relieve the disturbance of disease, instead of adding to it; and, during the progress of the case, great caution is exercised in the use of any remedies that may interfere with the rest and quiet so essential to the free operation of the recuperative powers, or that may so depress them that they

cannot act with sufficient energy to effect a recovery. The truth that the irritation of disease is often the great source of the exhaustion attending it, and that the physician should therefore be careful not to add to it by his remedies, is now quite fully appreciated. Commonly, it is true, some disturbing treatment is required occasionally during the progress of a case; but it is managed with caution, and generally quieting remedies are used in combination, so as to render the disturbance as slight as possible.

The change of which I have been speaking has done for medicine what the introduction of the art of healing by the first intention has done for surgery. The irritation of the disturbing modes of treatment so prevalent during the reign of active medication had the same effect upon internal maladies that the irritating ointments had upon the wounds into which they were inserted so universally by surgeons before the time of Ambrose Paré; and the improvement in both cases consists in a return to the simplicity of nature.

Perhaps there is no remedy in the use of which there has been so much change as bleeding. It was during the first quarter of this century, and even for some years farther on, a common remedy in all febrile and inflammatory diseases. I have already alluded to the views of Dr. Gallup in regard to this remedy. Although there were few in this country whose views were as extreme as his were, bleeding was everywhere a favorite remedy with the profession. In England, Armstrong and Southwood Smith were the prominent advocates of bleeding. The strong views of the latter — so earnestly, skilfully, and I may say beautifully, developed in his book on fever — were very captivating to all enthusiastic minds; and his book had for a time a wide influence, both in England and in this country.

Bleeding was popular with the people as well as with the profession. In almost all cases of accident, it was practised as a matter of course. In pregnancy, it was resorted to as

the established remedy for various inconveniences and complaints, that we now find are easily removed by less formidable means, or that commonly had better be borne than be removed, if bleeding be the only thing that can remove them. It was the custom also with many to be bled occasionally, in order to guard against attacks of disease to which they supposed themselves liable. This was practised especially in the spring.

This very common resort to bleeding, both as a remedy and as a preventive, is now abandoned. This remedy has, with others, been subjected to discriminating limitations; perhaps, from the influence in part of popular prejudice, it has been in some quarters too much given up, especially local bleeding. Whenever inflammation exists in an important organ in any marked degree of severity, this remedy is applicable, — general bleeding when there is sufficient constitutional affection to call for it, and the system is in a condition to bear the loss of blood; and local bleeding when the circumstances of the case do not warrant general bleeding.

Bleeding, it is to be remembered, is a remedy that is calculated to allay the irritation of disease; and it never adds to it when it is really applicable, and is not made use of to an improper extent. It is therefore, in some cases, really not as objectionable as certain remedies that are substituted for it in order to avoid exhaustion. It is often better to reduce febrile excitement or inflammation by this quiet remedy than to do it with remedies that may exhaust the vital energies by a series of impressions which are depressing, and at the same time irritating. I am persuaded that there is often too little fear of this result from such impressions on the part of those who have great fear that bleeding would produce it. Remedies of the kind alluded to, when pushed too far, may cause an exhaustion as irremediable as that which is produced by an inappropriate bleeding.

Having thus noticed the change in practice in relation to

the use of certain remedies, I pass to consider the changes which have taken place at the same time in the modes of treatment in some diseases.

I have already alluded to fever in speaking of remedies ; but the change has been so great in the management of this disease, that it deserves something more than a passing notice. An active interference, by means of bleeding, mercury, purgatives, emetics, antimonials, &c., was at one time the general practice ; it being supposed that such remedies could shorten, or even arrest, the disease. There were few that suspected that the prostration which was so apt to ensue in the progress of the fever was in part the result of the medicines used in the beginning. But a great change has taken place in the treatment of fever. Active measures are, for the most part, abandoned. Quieting measures predominate ; and great caution is exercised in avoiding any thing which may exhaust the strength by irritation or by direct depression. If the fever be uncomplicated, the treatment is simple ; and generally it is its complications only that are attacked with active remedies ; and this is done cautiously.

This change is the result of several concurring causes. First, the consideration of the character of the disease contributed to this result. So long as it was supposed that fever was a disease that could be broken up, or be materially shortened, by active treatment, this treatment continued in vogue ; but, with the alteration of the views of the profession in regard to the character of the disease, the treatment became less active. And most medical men now feel, in regard to active treatment, as Dr. Pitcairn did, who said, when he was asked his opinion of some treatise on fever, "I dislike fever-curers. A fever can be guided : it cannot be cured."

A close observation of the effects of remedies contributed to this result. Thus, as quoted by Dr. Hale in his investigation of the typhoid fever of New England, Dr. Bright said in 1827, that he had "almost always found that the

small doses of antimonial remedies usually administered, as a part of the diaphoretic plan, do harm where any decided tendency to irritation of the bowels exists." Similar observations were made by others, both in relation to this and other irritating remedies.

But, thirdly, after a time, pathology brought in its investigations to confirm the observations made by Bright and others, in regard to the unfavorable effect of some of the active remedies. Among the pathological appearances found the most common is inflammation of Peyer's glands. This inflammation must, of course, go through a certain process, before a recovery can be effected. It is not an inflammation of a rapid character; and so time must be given for the recovery. We should be exceedingly careful not to aggravate the inflammation by our treatment; and, although nothing very direct and decided can be done to lessen it, we may so alleviate the general irritability, and that of the intestinal canal, as to make the inflammation go through its course with ease, without coming to ulceration, or to insure a successful termination to it even when it is severe enough to reach this result.

The practice of Broussais, although it was based upon a false theory, did essential service in bringing about this change in the treatment of fever. The results of the displacing of irritating remedies by his leeches and gum-water showed very clearly that such remedies were not required,—at least, as the ordinary means of treating the disease.

A change very similar to that which has occurred in the treatment of continued fever has occurred also in the treatment of the exanthematous diseases. This is especially true of scarlatina. Such remedies as bleeding, calomel, antimonials, purgatives, &c., so common in the treatment of this disease in the first part of the past fifty years, are now used but sparingly, and with much caution. During the period of active medication, various modes of treatment were lauded by their advocates, each mode having some prominent reme-

dy, accompanied by some lighter remedies as auxiliaries. Thus Dr. Fothergill used chiefly stimulants ; Dr. Southwood Smith, bleeding ; Dr. Armstrong, calomel ; Dr. Currie, the affusion of cold water.

The mortality from this malady would undoubtedly have been less, if it had been left wholly to nature's recuperative efforts, instead of being subjected to such active and exclusive modes of treatment. And yet all the remedies of these modes are applicable to some limited extent ; and the true practice is to use, for the most part, negative measures, and remedies that quiet irritation, with the occasional employment of the active remedies alluded to, when the circumstances of the case call for them by indications that are clear and decisive. And this is the view of the treatment of this disease which is now adopted by the great majority of the profession.

This malady belongs to that class to which Dr. Bigelow gave the name of self-limited diseases, which he thus defines : "By a self-limited disease, I would be understood to express one which receives limits from its own nature, and not from foreign influences ; one which, after it has obtained foothold in the system, cannot, in the present state of our knowledge, be eradicated or abridged by art, but to which there is due a certain succession of processes, to be completed in a certain time ; which time and processes may vary with the constitution and condition of the patient, and may tend to death or to recovery, but are not known to be shortened or greatly changed by medical treatment."

This definition applies more strictly to some diseases of this class than to others ; or, in other words, the limits differ in definiteness in the different diseases which may properly be said to belong to this class. Thus small-pox and measles are very definite in the period which each occupies in its processes ; scarlatina is rather less so ; and continued fever still less. Dr. Watson considers the latter to occupy gene-

rally about three weeks :¹ but sometimes it is shorter, and often is much longer than this ; and, when it is prolonged, it appears generally to be a continuation of the same disease essentially, and is not, as is the case with scarlatina when prolonged, merely a prolongation of results rather than of the disease itself. It is upon the circumstances that govern these continuations that medication can sometimes exert a decided influence ; and the more so, the less definite is the natural period of the disease.

The progress which the profession has made in the therapeutics of this class of diseases is much greater than is commonly supposed. Physicians are hardly aware of the position of most of the profession some twenty-five years ago, in regard to their nature and treatment. It was such that Dr. Bigelow, in his discourse on self-limited diseases in 1835, seemed to have some distrust as to the reception his ideas would meet with. His language was therefore cautious. He spoke of the object of his discourse as being “to endeavor to show the existence of such a class of diseases ;” and he says, “In proceeding to enumerate more precisely some of the diseases which appear to me to be self-limited in their character, I approach the subject with diffidence. I am aware that the works of medical writers, and especially of medical compilers, teem with remedies and modes of treatment for all diseases ; and that, in the morbid affections of which we speak, remedies are often urged with zeal and confidence, even though sometimes of an opposite character.”

Although there were many at that time holding more or less the same views with Dr. Bigelow, yet it was quite common among medical men to speak of his discourse as showing that he was unduly sceptical in relation to the powers of medicine. But, at the present time, these views are those of the profession generally ; and, in carrying

¹ As the distinction of continued fever into typhus and typhoid fevers is still a subject of dispute, I have everywhere in this Essay spoken of it as one thing.

them out in the treatment of disease, a very great advance has been achieved in the discriminating limitations which have been placed upon the use of remedies in this class of maladies.

In that most common of all chronic diseases, phthisis, the change in treatment, since the reign of active medication has passed away, has been of the most decided character. Bleeding, emetics, antimonials in small doses, digitalis, &c., were quite common remedies in the treatment of this malady in the first quarter of this century. Although, as long ago as 1789, Dr. Thomas Percival developed the true pathology of phthisis, taking the ground that "inflammation is perhaps only an occasional concomitant" of the formation and softening of tubercles, yet the general practice in this disease continued long after that to be founded upon the idea that there was inflammation, or at least congestion, to be combated from the very outset. But now this active mode of treatment is abandoned. It is a general conviction in the profession, that little comparatively is to be done by medication; and most of the remedies that are used are such as are calculated either to give tone to the system, or to quiet irritation. But perhaps, in avoiding the errors of our predecessors, we have gone too far. Inflammation is, I am persuaded, too much ignored at the present day as a concomitant of the essential pathological process in this disease; and local bleeding and counter-irritation are too seldom employed. There is danger of our having an exclusive mode of practice in this respect, and thus lessening that "advance in favor of humanity and scientific medicine," which has certainly been secured in the marked abandonment of drugs in the treatment of this malady.

Perhaps there is no disease in which there has been so thorough an abandonment of active medication as in delirium tremens. For a long time, the doctrine of the profession was that promulgated by Dr. Sutton,—that the patient must sleep or die; and that the grand means of securing sleep

was opium, which was supposed to be ordinarily needed in large doses. The profession were right in regard to the first clause of this doctrine. The excessive agitation of the nervous system would, of course, wear out the patient, unless it could be brought to an end, and the patient could sleep. But they were wrong in regard to the necessity of opium to produce this result. The agitation can be quieted by other means, as alcohol, for example ; and even negative measures will often answer the purpose, — the disease coming to an end from the mere withdrawal, as far as is possible, of all excitement of body and mind. To Dr. Ware, of Boston, belongs the honor of having first distinctly announced the truth on this subject, which he did nearly thirty years ago. Following up his observations, he became satisfied even that the sleep in which the disease terminated was much oftener a spontaneous termination than the result of the operation of opium.

Since the investigations of Dr. Ware, various plans of treatment have been adopted. Dr. Klapp, of Philadelphia, relied almost entirely upon emetics. He states, that, of 51 cases, opium and alcohol were used in only one case ; and that only one death occurred, which was caused by epilepsy. The alcoholic treatment, of which Dr. Gerhard, of Philadelphia, is a prominent advocate, is represented as equally successful. He reports, that, of 162 cases treated in one year in the Philadelphia hospital, only one died ; and he had been treated with opium previous to his admission, and died when he had been in the hospital but a few hours. Dr. Dunglison reports similar success in the treatment of 84 cases in the Women's Lunatic Asylum ; the treatment being "entirely eclectic, and in many cases expectant." Opiates and stimulants were given in but few of the cases. Emetics were used in some cases. The patients were shut up in a darkened room, and kept quiet ; easily digested food was prescribed, when the stomach would retain it ; and the bowels were kept open by gentle cathartics. This consti-

tuted the treatment. Of the 84 cases, all recovered but one. In this case, the patient had been treated for a week before admission, and was not seen at all by Dr. Dunglison.

Although these statistics are not extensive enough to settle many points in regard to treatment, there are some few points which they do settle very clearly. First, that opium, so far from being a *sine quâ non* in the treatment of this disease, is not ordinarily essential to effecting a cure. Second, that the recuperative power of nature is the chief agent of cure. Thirdly, that the disease is almost always recovered from, if it have no other disease complicated with it. Fourthly, that there is no one plan or mode of cure which is exclusively applicable.

There are some things which these statistics do not prove, some of which it is well to notice. They do not prove that opium is an improper remedy; neither do they show that any of the other means, which have been made the prominent measures of the several modes of treatment, are inapplicable; neither do they indicate the relative value of these different means, or the circumstances which should govern us in their application. Mere statistics alone cannot settle such points; they must be settled by minute and careful observation of cases; and general statistics can only be auxiliary to such observation in doing it. It is this observation, that, under the guidance of established general principles, is determining, in the experience of multitudes of careful and scrutinizing practitioners, the relative value of the different remedies; and, so far as we can get at the verdict of this observation at present, among the positive means of cure, opium in moderate doses is the most important. If there could be an accurate comparison made between the results of the present discriminating practice in this disease, and the heroic opium practice of former days, it would undoubtedly show that the approach which has here been made "towards the rational and successful management of disease, without the necessity of drugs, is an advance in favor of humanity and scientific medicine."

There are some diseases in regard to which there is still a struggle going on in the profession in relation to modes of treatment, similar to that which so long prevailed in regard to some of the diseases which I have noticed. Such are yellow fever and cholera. There are the same discrepant testimonies in regard to the modes of treatment in these, as there have always been about exclusive modes in other maladies; and probably the result which has commonly been reached will be reached here. The profession will soon come to the belief, that medicine has less power over these diseases than has been supposed; that no one mode of treatment is universally applicable; that nature is to be trusted quite largely; and that the curative means must be employed in obedience to general principles, instead of narrow theoretical notions.

The movement towards such a result is very strong, of late, in regard to yellow fever. Bleeding, stimulants, quinine, calomel, the tincture of muriate of iron, &c., have each had their warm advocates; but active medication of these various kinds is beginning to be given up. Thus Dr. Fenner, of New Orleans, in his report on the epidemics of Louisiana, in the "National Transactions" of this year, says, "In respect to treatment, I feel authorized to say, that the general opinion of the profession in this region now is, that we have hitherto been giving too much medicine in yellow fever; in other words, we have been taking it out of the hands of nature, and trying too hard to *cure* it: whereas all that seems necessary to be done is to assist nature in her conflict with the febrific cause." So also Dr. Cain, of Charleston, says of the profession of that city, that they have "generally settled down upon the opinion long since promulged by Pitcairn respecting common continued fever (typhus and typhoid); viz., that yellow fever cannot be *cured*, but *may be conducted* to a favorable termination."

There are some diseases, the treatment of which is as active now as it ever was. Such, for example, are colic and

intermittent fever. In the latter disease, quinine is often given much more freely than it formerly was ; and by many with little regard to those circumstances which, it has been thought, should limit its use, and in some cases altogether forbid it. But the limitations, which the varying circumstances of the different cases should place upon the administration of this effective remedy, are not yet ascertained with sufficient definiteness ; and much is undoubtedly to be yet learned in regard to its use by careful and extended observation.

In the inflammations, active medication is much employed at the present time, but less boldly than formerly, and with more discrimination in regard to the applicability of the various means in different degrees to different cases. When inflammation occurs in connection with other diseases, as fevers and the exanthemata, it cannot be attacked with active remedies as freely as when it occurs alone. Especially is this so when the inflammation forms a part of an epidemic disease. Good examples of this are found in yellow fever and dysentery. When dysentery is sporadic, — that is, when it is an inflammation simply, with the accompanying fever for the most part symptomatic, — it is much more amenable to remedies than when it is an epidemic dysenteric fever ; the inflammation in this latter case bearing to the existing fever the same relation that the inflammation in yellow fever does. In both diseases, the inflammation and the fever are results together of the cause or causes producing the disease.

This leads me to remark, that, commonly, active medication is much less admissible in those acute diseases which depend in part upon some occult cause, than in those which result from causes, the operation of which we to some extent understand. If this fact had been more recognized by the profession, we should not have had such bold and various treatment, with all its discrepant testimony, in the epidemics that have from time to time appeared ; and their ravages would not have been so extensive and severe.

I have thus portrayed to some extent the change which has taken place in medical practice during the past twenty-five or thirty years. Just how far this change has been a real "advance in favor of humanity and scientific medicine," it is impossible to estimate; but that the advance has been a great one is very evident. The deliverance from the suffering that formerly came from fruitless medication is of itself no small gain. The amount of life saved would be seen to be very great if we could obtain correct statistics on this point. But, besides this, there is great gain in many cases in the actual shortening of the term of sickness, and in the more clear convalescence which is established; for an indiscriminating active medication is apt to make long and bad cases of mild attacks of disease, and to leave patients, on recovery, with a shattered system and morbid tendencies, ready to be lighted up into active disease on the application of any exciting cause.

We sometimes have the opportunity of seeing the bad effects of indiscriminating active medication exhibited in the most palpable manner. Not only does quackery furnish us with such opportunities, but we meet with them occasionally within the bounds of the profession. I will give but a single example. There was at one time much noise made in various parts of New England about a disease which was called typhus syncopalis. That such a disease did exist, I will not deny: but, at the same time, we have the most abundant and reliable evidence to show, that much of what was called by this name was the product of the remedies administered for its relief; in other words, that it was often in fact a brandy and opium disease. I will not detail the evidence, but simply state it in the general. 1. In many cases, the discontinuance of the remedies effected a solution of the symptoms. 2. A simple and mild treatment relieved in a very short time cases which had the same symptoms with those which, under the brandy and opium treatment, became severe and protracted cases. 3. In some instances,

all the cases of the so-called typhus syncopalis occurred only in the practice of those who pursued this mode of treatment, instead of being distributed among the different physicians in the same locality. The evidence on the two first points is abundant and various; and the whole proves beyond a question, that a continued use of stimulants and opiates can, under certain circumstances, produce a morbid condition resembling that which has been described as sinking typhus.

As then we see, in this and in some other cases, an inappropriate positive medication almost entirely creating the disease which it is supposed to cure, we can have some conception of the extent of its varied influence in different diseases, more or less modifying and aggravating them. And the fact that it is not more palpably destructive of life is owing to the influence of the recuperative powers of nature, which are ever ready to do their work, and commonly do it very effectually as soon as art's busy meddling with her operations is given over.

In dismissing this topic, I remark, that Homœopathy has derived much of its popularity with the people from the manifestly bad results of rival modes of active medication; and that undoubtedly many of those practitioners of this form of quackery, who were once in the ranks of our profession, are really less unsuccessful than when they practised what is called regular medicine, because now they leave to nature what they once undertook to do by their bungling over-medication.

I propose now, in pursuance of the plan of this Essay, to enter somewhat upon a comparative estimate of the value of the different kinds of curative means, which have been so variously brought into view in commenting upon the changes that have taken place in medical practice. This is necessary as preparatory to the development of those principles which should guide us in the use of these means.

The most important of all these means is the recuperative

power of nature. All our means of art have little influence in comparison with this. Well is it said by Sir Gilbert Blane, that "the benefit derivable to mankind at large from artificial remedies is so limited, that, if a spontaneous principle of restoration had not existed, the human species would long ago have been extinct."

But why do we call this recuperative power *our* means of cure? Because we can use it. We can modify and direct its efforts; we can remove obstacles out of the way of its action; we can put the system into a condition to receive the full benefit of its efforts. A large part of the physician's duty is thus to be waiting upon nature; and, even when he uses active measures, they must commonly coincide with her efforts, or they will do harm. It is seldom that he is called upon to go counter to her operations, and then only temporarily.

This is *our* means of cure much in the same sense that the wind is the sailor's means of bringing his ship safely into harbor. With his appliances, he so adjusts his vessel that this natural power shall effect the purpose, as the physician adjusts the circumstances of his patient so as to let the natural powers in his system carry him safely into convalescence. The comparison might be followed out, without bordering in the least upon the fanciful, in other particulars; but they are so obvious that it need not be done.

Next in importance in the treatment of disease is the class of quieting and comforting means and measures. Disease is ordinarily accompanied by turmoil and suffering. These it is important to allay, in order that the recuperative power may act easily and effectually. The means of doing this are widely various. Some of them are negative in their character. Rest, which is often so manifestly necessary in the practice of the surgeon, is as necessary in the practice of medicine. There is quite a common failure among practitioners on this point. The neglect of it often counteracts in part, sometimes wholly, the curative influence of remedies.

Many a patient dies because the physician has not given him rest. Especially is this true of mental rest; and, when such neglect occurs, the dereliction of duty is no less than if the physician had carelessly allowed the patient to take a poison that killed him. He has, in fact, allowed him to take a poison; and, though it be a mental one, it has proved as fatal as if it were a poison introduced into the stomach.

There are various drugs which are used for the purpose of securing rest, and relief from suffering. Opium is not only at the head of this class, but it is *the most important of all drugs that are employed in the treatment of disease*. It helps the physician in giving his patient that rest which is so effectual a means of cure. Its direct influence upon disease constitutes but a small portion of its usefulness. It is in its indirect influence that it has so wide and varied a curative agency. By its relief of pain, and its calming of disturbance, it saves from the exhaustion and aggravation of disease, that are so certain to result from continued irritation; and enables the recuperative energies to do their work quietly and effectually. It also, conjoined with other remedies, makes them act kindly, when they otherwise would occasion so much irritation that they would do harm rather than good. In these ways, this remedy is constantly of use in the treatment of disease. It exerts ordinarily so quiet and gentle a ministration, that we are apt not to be aware of the great amount of influence that comes from it.

In the reign of active medication, it was common to speak slightly of palliative remedies, in distinction from those which were supposed to be radical in their influence upon disease. It was not only the popular notion, but it was to some extent the belief of the profession, that opium never cures. In those cases where such active remedies as bleeding, mercurials, &c., were used, these were supposed to effect the cure, while opiates merely relieved the pain and restlessness. The fact that in doing this they had much to do with the cure was but imperfectly recognized. Much less was it

seen, that, in many cases, nearly all that the physician can do is to allay disturbance and relieve pain by opiates and other means, in order that nature may carry on her curative operations quietly and effectually. And here I cannot forbear to remark, that opium is of much more value than many suppose in quieting the irritation of commencing disease. Its use is too often deferred till certain impressions, deemed to be a necessary preparation of the system for the action of this remedy, are made upon it. It is indeed true, that opium may be so used as to cover up merely the smouldering fire of disease ; but this is only when other means, which should be used at the same time, are neglected.

There is no truth better established than that whatever palliates has, in doing this, a tendency to cure ; and, taking into view the whole range of disease, quieting and comforting influences have more to do with effecting recovery than those which are disturbing. Even the remedies which for the moment disturb, often do more towards the cure, by the relief which they at length afford to disturbance or suffering, than by any direct effect which they have upon diseased action. This may be said of all remedies that remove sources of irritation. And the lighter means of contributing to the relief of the turmoil and distress of sickness are not to be neglected. Refreshing influences acting upon the morbid sensibilities, genial mental influences, even so small things as the smoothing of a pillow, often contribute much to the recovery, and are sometimes essential to it.

I will not go further in the classification of our means of cure according to their relative importance. It is sufficient for my purpose to show that nature's salutary efforts, and the quieting means which have so much influence in favoring these efforts, stand in importance far above all remedies of a disturbing character.

Before, however, dismissing this subject, I will remark briefly upon the prominence which should be given to simple means in the treatment of disease. The judicious application

of these is often neglected, while the physician is busied with the administration of remedies, the operation of which is perhaps involved in obscurity. The most successful practitioners are those who take the simplest views of diseases and their remedies ; practising according to the dictates of a good common sense, taking that term in its highest meaning. On the other hand, those who are captivated with recondite views of the *modus operandi* of medicines are unsuccessful practitioners. A young physician once asked an old practitioner,¹ who had acquired by his practical good sense a wide reputation, not merely with the public, but with all the profession in his neighborhood, what his principles of practice were ; expecting to hear from him some very profound remarks on the subject. But the sagacious old man replied, "My principles are very simple. If the patient is hot, I cool him ; if he is cold, I warm him ; if there is pain or restlessness, I relieve it ; if there are irritating matters, I evacuate them ; if any secretion is scanty, I try to make it free. These are some of my most important principles."²

To such a physician, the more simple the means of cure, the better. Cold water, which can be used so extensively and so variously, is to him often a remedy of greater value than any drug that can be administered.³ He makes

¹ The late Dr. Amos Twitchel, of Keene, N.H. This eminent physician used to relate often an anecdote of himself, and his preceptor, Dr. Nathan Smith, which is very instructive as to the caution necessary in drawing inferences from single cases in regard to the efficacy of remedies. "In the earlier part of my practice," said Dr. Twitchel, "I made use of a certain preparation of silver, and wrote to Dr. Smith, saying, 'I can cure epilepsy; at least, I have done it.' Dr. Smith wrote in reply, 'Do it again.' But," added Dr. Twitchel, "I never have done it again to this day."

² The grand improvement which Sydenham introduced in the treatment of small-pox consisted simply in the application of plain principles of common sense. And I know of no fact which shows more strikingly the prevalent disposition to overlook these, and to grasp at something beyond them, than the slowness with which his principles of treatment in this malady obtained a foothold in the profession.

³ Neither the efficacy of this remedy, nor the variety of modes in which it can be applied, is appreciated generally by the profession ; and from want of caution, and a disregard of the simple principles which should regulate its application, it is very often used injuriously, especially when its application is left to attendants with vague and unintelligible directions, as is too often the case.

much account of such matters as friction, external applications of various kinds, the regulation of the temperature of the room, of the amount of clothing, ventilation, cleanliness, &c. He attends also to the mental influences that are brought to bear upon the patient. He does not consider himself the mere doser of the body ; but he regulates the mental doses, so to speak, that are administered, sometimes considering these of more importance than the drugs that he gives. He feels bound to take charge of every thing that can in any way affect the case, and is satisfied with nothing short of absolute control of the sick-room.

I pass now to the development of certain principles, by the guidance of which, in the use of curative means, we may secure the advance that is contemplated in the proposition which is the subject of this Essay.

It is a grand axiom of Chomel, that it is the second law of therapeutics *to do good* ; its first being this, — *not to do harm*. This axiom, however, does not go far enough. It would be better if there were added to it the words, *and to prevent harm from being done*. An active interference is demanded of the physician to shut out all injurious influences. His duty in this respect, as I have before said, is as positive as in the administration of remedies.

But I will go more into particulars. I lay it down as a rule, fairly deducible from the views which I have presented in relation to therapeutics, that *no active medicine should be used in any case, unless the evidence is clear that it will effect good*. This is in entire opposition to the old axiom, *Melius anceps remedium quam nullum*, — an axiom which, though time-honored, has been largely destructive of life, and has hindered greatly the progress of therapeutical science. This axiom would indeed be applicable if disease were cured only, or even chiefly, by medicine ; but, as the recuperative power is the chief agent of cure, there is in the use of all doubtful means great hazard of interfering with its salutary efforts.

Substantially, this rule has been adopted by sagacious men in other matters than medicine. After Lord Chatham had, upon some occasion, criticized the doings of the ministry, it being said in their defence that the error charged upon them arose from a want of information, he said, in reply, that it had ever been the rule of his life, whenever he did not know what to do next, to do nothing. Especially applicable is such a rule in medicine: for besides the fact, that in the strife of disease there are many and complicated agencies at work, some of which may be unknown (making, therefore, a throw at a venture peculiarly hazardous), there is one powerful agency — the recuperative power — always working for good; with which it is exceedingly important that the physician should not interfere, and to which he had better intrust the welfare of his patient, than employ expedients of a doubtful character.

But are there no exceptions to this rule? There are some; but they are few. Some diseases furnish exceptions: they are diseases in which, thus far, there has been no cure in nature, nor any found by art. Hydrophobia is an example. Here there is properly room for experiment with remedies.

So, too, there are occasionally cases of disease that is ordinarily curable, in which it is manifest that the patient must die, unless some active interference of art can save him. Here a doubtful remedy, from which there is some reason to hope, is admissible: but, to warrant its use, the case must be a clear one in regard to the prospect of a fatal termination; and it must be remembered, that cases which seem to us to have a fatal tendency, almost beyond a doubt, sometimes recover from causes that we do not understand. And I may remark in this connection, that the capabilities of nature are often not sufficiently appreciated in severe disease, as such cases show us. Cases in which such unexpected recoveries occur are for the most part rather indefinite in their character. They are cases in which a physician that always wishes to have clear reasons for what he does is in

doubt what to do ; that is, so far as any remedy that will act with any directness or efficiency upon the disease is concerned ; and so, in obedience to Lord Chatham's maxim, he does nothing. He watches the movements of the case, counteracting, so far as he can, the tendency to death ; sustains the exhausted powers ; quiets irritation ; and awaits the result. A busy interference in such a case would frustrate the salutary efforts of nature ; unless, as a mere matter of chance, the physician should strike upon a plan that coincided with these efforts. There are really, then, very few cases of the kind designated in the beginning of this paragraph, in which it is proper to disregard the rule laid down.

I would consider as exceptions to the rule some mild cases of disease, in which it might be proper to try the effect of doubtful remedies : but, of course, such experiments should be very infrequent, and should be very carefully made ; and great caution should be exercised in drawing inferences from them in regard to the applicability of the remedies to grave cases.

Some would, perhaps, be disposed to exclude from the operation of the rule cases of chronic disease. I see no reason why they should be excluded. There is as real, though ordinarily not as great, hazard in indefinite, aimless dosing in chronic, as there is in acute, disease.

Perhaps it will be thought that so strict a rule will prevent us, in many cases, from doing good, which, without this rule, we might, perchance, be able to do. This is undoubtedly true of some cases ; but the number of them will be very much less than the number of cases in which this rule will save us from doing harm.

Venturesome medication is captivating, especially to the young and enthusiastic practitioner ; and the charm is enhanced by the occasional brilliant achievements with which it is attended. Many of these achievements, however, are only apparent, being erroneously attributed to the remedies, when they are really the result of nature's efforts, and have been

effected, perhaps, in spite of the agencies to which the credit is given. While the bold practitioner has this brilliant but often false show of success, the better results of the practice of the cautious physician commonly make but little display; yet, when he does attempt to produce decisive effects by his remedies, so definite is his aim, that the result may be calculated upon almost with certainty.

This leads me to say, that the adoption of this rule will relieve the practice of medicine, to a considerable extent, of its uncertainty. The practitioner, always having definite aims, and generally accomplishing clear results, will become exceedingly exact in his observation; and his recorded experience will be of great value. With many observers at work in this way, noting down the results and comparing their records, the circumstances which should regulate the use of remedies will be accurately and extensively ascertained; and therapeutics will become immeasurably more definite than it now is.

Perhaps some will complain, that the rule which I have laid down hems in the practice of medicine within too narrow limits. But if the physician takes the broad view which I have presented of our means of cure, and attends to the regulation of them all, both the negative and the positive, he will find enough to do, even with the strictest application of the rule; and then the results of his definite observations under this rule will enable him, as his experience increases, to widen the range of his active interference in the treatment of disease.

It will undoubtedly be thought by the advocates of a bold practice, that the rule which I have stated lowers the dignity of the physician's office by restricting so much his *active* agency in combating disease; but, so far from this, it is really enhanced. Great skill is often required to do aright the little that is to be actively done; for there must be accurate and painstaking discrimination in order to distinguish between the salutary efforts of nature and the symptoms of

the disease, and to adjust the agencies which he employs so that they may coincide with those efforts, instead of thwarting them. It is a far more complicated, and therefore more difficult, plan of practice, if faithfully carried out, than that which is commonly pursued by those who are in favor of an active medication. It admits of no stupid and indolent submission to routine, nor of that mere show of industry which attends the practice of the theorizing practitioner. It calls for thorough, patient observation, in order that all the circumstances of every case may be properly regulated, and that every opportunity of exerting a decisive good influence by remedies may be promptly secured. There is sometimes a necessity for very active medication ; and the physician is to estimate carefully the degrees of necessity in different cases. A truly rational practice takes so many points into view, and varies so much its adjustments to the infinitely varied necessities of the different cases, that it affords scope for the exercise of the very highest powers of mind.

Very dignified is the stand sometimes taken by the discriminating physician, when, after a careful survey of all the circumstances of a case, he comes to the conclusion that the patient will have a better chance of recovery if he for the most part be let alone, than if his case be actively treated. The disease may be violent in its character, seeming to the common observer to call for the most active interference of art, and the importunities of the friends of the patient for such an interference may be exceedingly urgent ; and yet he remains firm to his purpose, using only such palliatives as may assist nature in weathering the storm. It is truly a "masterly inactivity," of which a frivolous and indiscriminating mind is wholly incapable. It is in strong contrast with the fretting and vacillating course which the indefinite doser is apt to pursue in such a case.

In following the rule which I have laid down, it is not required of the practitioner that he should know with abso-

lute certainty that his remedies will produce the effects that he contemplates. All that is intended is, that he must have good evidence that they will in all probability do so. Medicine is not an exact science, but is ranked among the inexact and conjectural sciences. Some go so far as to say that a good practitioner is only a good guesser; but this is a gross libel on the character of medical evidence. The conclusions of the rational physician are founded upon a careful examination of evidence, which is often so complicated that it requires great skill to unravel it. There is difficulty, it is true, which leads the superficial and indolent to guess: but it calls forth the highest powers of observation and reasoning in the thinking and industrious; and, with the exercise of proper caution, they arrive at conclusions which are clear and safe guides for them in their practice.

The inquiry, then, naturally arises here, what the nature of the evidence is upon which the physician must rely. There is great mistake often on this point. We see this in the discrepant opinions which are sometimes given by eminent physicians in regard to the use of active means in the treatment of the same disease. The relation of the remedies to the morbid condition fails to be recognized by either party; else there would not be such opposition of views. If, for example, stimulating and depressing remedies are both used under the same circumstances by different physicians, and both parties make such show of success that it is difficult to decide between them, both must be in error in regard to the nature of the evidence to be relied upon in discovering the relations of remedies to the varying circumstances of disease; and the error is a radical one. It is a failure in the very foundation of practical medicine.

The evidence which we have in regard to the action of remedies upon disease is twofold. First, there are some remedies that have a relation to disease which we do not understand. We only know that they cure the diseases to which they have this relation. A very marked instance of

this kind is the relation of cinchona to intermittent fever. We may theorize in regard to its *modus operandi*; but we really know nothing about it. We only know that it arrests the disease.

But the remedies which have this occult but definite relation to disease are few in number; and our evidence in this direction is therefore very limited. Most of our evidence is in regard to remedies that have altogether a different relation to disease. They are remedies which are found to produce certain effects upon the system; and it is from a knowledge of these effects that we judge of their applicability in individual cases. In other words, we know something of their *modus operandi*; and this knowledge is, or should be, the foundation of our use of them in the treatment of disease. Thus, it is what we know of the effects of bleeding upon the circulation that guides us in the use of that remedy for the relief of fever or inflammation. So the effect which we see calomel produce as a stimulator of the secretions is chiefly the ground of our use of this drug in many forms of disease. Examples might be multiplied; but these are sufficient.

The second rule, then, which I would lay down for our therapeutics, is, that *the practice in each case should be based mostly upon what we know of the modus operandi of remedies*. I say, upon what we *know*; for many attempt to go beyond what is known, and grasp at the occult in the operation of remedies, making their suppositions in regard to it the basis in part of their therapeutics. There is no objection to such suppositions if they are treated as such. They may indeed lead to some discoveries in relation to the action of remedies; but when they are considered as established truths, and are acted upon in practice, they are legitimate sources of error. Nothing but what is actually known should be the basis of action. It is only by a strict adherence to this rule that medicine can be redeemed in any good measure from its uncertainty. Mere speculation, when it is mingled with our

actual knowledge, makes it uncertain and confused ; and this result may always be seen in the practice of the physician who is captivated with speculative views of the occult operation of remedies.

Even in the case of remedies that have a definite and almost specific relation to certain diseases, their use is to be somewhat governed by other known effects of these remedies, and their relations to other morbid conditions ; that is, in order to make their direct relation to disease always available, their *modus operandi*, in their indirect influence, must be well understood. Thus, in giving quinine as an anti-periodic, we must have some regard to incidental circumstances in the case, which, from other relations of this remedy, may essentially modify, or even prevent, its desired action.

It is such a knowledge of the *modus operandi* of medicines as I have indicated that gives us certain *general principles* of practice ; for these principles are but expressions of the relations of the several remedies, or classes of remedies, to different morbid states. It is in the application of these principles to the infinitely varying circumstances of individual cases that the rational practitioner exercises his skill ; the only exception being in the case of those few remedies that act upon disease in a manner to him wholly occult, and which some would call specific.

Although much is said about general principles, they have been greatly neglected by many practitioners. This is seen in the proneness, which has ever been so prevalent in the profession, to adopt fixed *modes* of practice. Obedience to general principles is inconsistent with the adoption of any exclusive treatment. It leads to a liberal eclecticism. If medicines were specifics, either wholly or partially, modes of practice would be proper ; but as nearly all remedies act indirectly upon disease, and the circumstances which should modify their application are almost infinitely variant, all the strifes which the profession has witnessed between opposing parties

in regard to modes of treatment have not only been useless, but they have materially impeded the progress of rational medicine. In most cases of this kind, while both parties were wrong, neither was wholly so. Very commonly, the remedies used by both are more or less applicable in the varied conditions which the different patients present.

I have said that the application of the general principles of therapeutics is to be greatly varied in individual cases, in obedience to their varying circumstances. Most of these circumstances are easily recognized: but some are discovered with difficulty, at least at the outset; and some are entirely hidden from view. These secret elements, existing in many cases, modify essentially the effects of remedies, and sometimes render improper the use of those remedies which the circumstances that are known in the case, taken by themselves, clearly call for. This suggests another rule of therapeutics; viz., that *we should be governed in our treatment of disease by the actual effects which we see our remedies produce.*

This very important rule is often disregarded. The physician who is fixed in the notions that he adopts is apt to disregard it, especially if he be given to theorizing. So, also, is the physician who, from indolence or lack of discrimination, readily falls into a routine of practice. On the other hand, there may be too great readiness to make changes in practice from supposed effects of remedies, or from too little patience in regard to effects which are expected. The judicious physician avoids both these extremes of fixedness and variableness.

It would take me into too wide a field to consider to any extent the causes which vary the ordinary action of remedies; but some of them it will be profitable to notice.

The idiosyncrasies which we occasionally meet with are such causes. An idiosyncrasy may be such in relation to a remedy as to call for much larger or much smaller quantities of it than are usually given, or it may be such as to forbid the use of the remedy altogether.

A variation of susceptibility under the influence of disease — a temporary idiosyncrasy, as it may be termed — is a much more common cause than the one just mentioned. The susceptibilities are always more or less altered by disease; and just in proportion to this alteration is that of the relation of remedies to the diseased condition. We see this strikingly exemplified in the large doses of opium which are borne in severe pain, and in the amount of cathartic medicine sometimes required in a torpid state of the bowels. These are palpable cases, familiar in the experience of every one; but physicians very generally are not aware how extensively the susceptibilities are changed in disease, and how wide a range of variation in the doses of medicine is required to proportion them accurately to the necessities of each case. It is a very common failure to give either too much or too little medicine. I am persuaded, that, in chronic diseases, there is often much harm done by administering remedies that are really appropriate in quantities that make too decided impressions upon the system. In many cases, a succession of gentle impressions from a remedy will do good, when ordinary doses of it would produce so strong an effect as to be injurious. Both in acute and chronic diseases, there is, in the common practice of physicians, altogether too little variation in the doses of medicine to suit the different susceptibilities of patients; and probably the doses are more often too large than too small.

There are some occult causes of disease which modify the action of remedies. This is especially true of epidemic diseases, as I have remarked in another part of this Essay. Although our general principles of therapeutics, deduced from the ordinary relations of remedies to disease, are applicable in such maladies, we cannot act upon them as freely as we can in diseases that are open and clear in their character. There is something in the disease beyond what we see, modifying the effects of remedies often in an unaccountable manner. We must use the remedies that we deem appro-

priate, therefore, with great caution, watching their effects, and depending very much upon what we observe of them to guide us in the further use of the remedies. We may find that the unknown substratum so affects the relations of the apparently appropriate remedies, as to make them mostly, if not wholly, inappropriate. The symptoms may be such, for example, as we very properly consider as calling for bleeding, and examinations after death may show a state of things that bleeding is ordinarily calculated to relieve; and yet that remedy may be really appropriate in but few cases, perhaps in none.

In yellow fever there is such an unknown substratum, preventing the physician from obtaining those satisfactory results which he obtains from active medication in most diseases; and it is because this fact has not been distinctly recognized that there has been so much contention in relation to modes of practice in this malady. Physicians have been reluctant to acknowledge how little they know of its nature, and have therefore applied their remedies with a bold hand; the advocates of each mode persuading themselves that they have better success than those who practise after other modes.

In commenting on the rules which I have laid down for our guidance in the use of remedies, I have indicated in incidental remarks what would be the effect of a full adoption of these rules upon therapeutics. One result would be a great diminution of the amount of medicine administered. There would be a considerable "approach towards the rational management of disease without the necessity of drugs." It would be an eminently "rational" change, because, so far from being a mere general diminution, it would be a diminution by reason of discriminating limitations; and, with this diminution, there would be, as the result of the same discrimination, a more active medication in some cases than physicians now practise. In short, there would be both a thorough discrimination in regard to the circumstances

calling for medication, and an accurate proportioning of the quantities of medicines to the necessities of individual cases ; the range of doses being undoubtedly much wider than is now realized in the general practice of physicians.

But there are circumstances which are so decidedly opposed to the adoption of these rules by the profession generally, that it will be difficult to effect it. There are obstacles existing both within and without the profession. The community, for the most part, have great faith in the efficacy of medicine. This is shown by the demand there is for quack-medicines ; which is so great, that their sale, with its enormous outlay in advertising and other machinery, constitutes one of the prominent branches of business in the community. It is shown also in the common language of the people in relation to the efficacy of medicines. They are inclined to attribute cures to particular remedies which have been used ; and their inquiry continually is, What is good for this and that complaint ? having the idea that remedies have specific relations to diseases. Many, it is true, talk of nature's powers ; but they evidently have indefinite notions on the subject, and suppose medicine to be necessary to the cure of any disorder which is sufficiently grave to be called disease. The general disposition is to demand of physicians an active medication ; and those practitioners who are fertile in expedients are most apt to secure a wide popularity. Homœopathy is satisfactory to its adherents only upon the ground that its infinitesimal doses are endowed with a wonderful power. It thus caters in the most effectual manner to the prevalent disposition, and secures to itself the credit which belongs to the recuperative power of nature, — the grand curer of disease.

In such a state of things, the physician is strongly tempted to accommodate himself somewhat to the expectations of the people. It is a work which few are willing to undertake, to go against the general current of popular sentiment, especially when it is a sentiment which it is peculiarly difficult

to correct. For this reason, the great mass of physicians are induced to administer more medicine than their uninfluenced judgment would dictate. The few who manfully resist the current find themselves obliged to use placebos to some extent, as means which are absolutely necessary to carry out their plans of medication with success. Especially is this the case with those who have not an established reputation.

But there are obstacles to the adoption of proper limitations of active medication, not only in the popular mind, but in the profession itself. There are obstacles in the intellectual tendencies of many practitioners. The *post hoc propter hoc* mode of reasoning in relation to remedies is not confined to the people; but it is also a common error among physicians. The general habits of the profession in the investigation of the effects of remedies are not such as they should be. This is seen in the prevalent readiness to use to a large extent at once any new remedies, and in the extravagant notions which, at the outset, many physicians entertain of their efficacy. There is collected, in regard to every new remedy, a mass of crude, incautious observations, which are paraded on the pages of medical journals; and these must be sifted thoroughly by a careful and continued experience, before its efficacy can be properly tested, and the circumstances which should govern its application can be ascertained. The result is, that much harm is done before the profession really become acquainted with the proper use of the remedy, especially if it be one of considerable power; and, from the fact that it had at the first an undeserved popularity, it is apt, after a little time, to sink in the public esteem below its real value. This process, which is passed through by every new remedy, shows that there is little appreciation, in the mass of the profession, of the difficulties of therapeutical observation, and of the necessity of such rules for our guidance in the use of remedies as I have laid down in this Essay.

Besides all this, it is for the interest of no inconsiderable portion of the profession to have the prevalent ideas of the power of medicine perpetuated. This is true of all followers of routine, who, in the present state of things, easily satisfy the expectations of the public. It is especially true of those who rely much upon the notoriety which they acquire by particular modes of practice. The adoption by them of the rules which I have laid down would involve the abandonment of their chief means of success. It is for their interest to discredit the efficacy of the recuperative powers of nature, and to have as much credit as possible given to their favorite means of cure.

But, notwithstanding the existence of these obstacles, I believe, that, substantially, the rules which I have stated as those which should govern our therapeutics are becoming more and more established in the profession. The tendencies are decidedly in this direction. Even in the community at large, there is, among its most intelligent portion, some movement counter to the general strong tide of public sentiment. There is not enough of it, however, to enable a physician to maintain his stand in giving uniformly sufficiently little medicine, unless he resort somewhat to placebos; and, although it is unpleasant to a high-minded man to do this, yet there is nothing derogatory to his dignity or honor in doing it occasionally, either to save himself from the irksome and useless labor of encountering the prejudices of bystanders, or to save his patient from the injurious effects of over-medication, which he might otherwise receive at the hands of another. The necessity of resorting to this expedient could be soon got rid of entirely, if prominent physicians of established reputation everywhere would make known, in their intercourse with their patients, their sentiments on this subject. It is time that physicians should, as we may express it, show their hand to the public more thoroughly than they have done. Intelligent men should be disabused by us of their errors in relation to the powers of

medicine, and should be taught the importance of other means besides drugs in the treatment of disease.

Medical men have a duty to perform in this respect, both to themselves and to the community, — to themselves, in placing our profession on the elevated ground which it ought to occupy ; and to the community, in redeeming them from the injurious and sometimes fatal effects of the over-medication which is still so prevalent. Until this duty is extensively performed, so as to exert a wide influence upon the public sentiment, the practice of our profession must continue to be, in the eyes of the people, more or less on the same ground with quackery, at least in some of its forms. It is only by imbuing the public with the views brought out in this Essay of the comparative value of our different medical means, and of the discrimination that is needed to apply these means aright in the wide range of disease, that the community will be led to bestow that regard upon the profession which is its due. So long as a large portion of medical men yield a real or apparent assent to the popular notion that drugs are the great means of cure, and reputable physicians here and there appeal to this notion by giving undue prominence to particular remedies and to special modes of practice, the change in therapeutics, which I have indicated, must take place very slowly.

From what I have said, it is plain that the chief hindrances to this change are in the profession itself. This we should clearly understand, that we may make proper efforts for their removal. It is of little use to war against the quackery without, so long as we harbor in the profession influences that tend to place it on a level with quackery. These influences must be exposed, and at least neutralized by counter-influences, if not directly combated ; and the movement which has been for some time so decidedly manifest in the leading practical minds of the profession, towards a very strict discrimination in medication, must be in every way encouraged.

There is much more to favor this movement now than there was when it was first set on foot. There were formidable obstacles to it then, which are now removed. The age of theorizing is past; and practical medicine is thus relieved of one of the principal hinderances to its advancement. The last general theory of medicine which gained an extensive hold on the profession was that of Broussais, — a theory which, quite in contrast with its predecessors, soon passed away under the advance of a strict and rational observation, imbittering sadly the last years of this great man's life; and the reign of observation is now so fully established, that no general theory of medicine can ever again be dominant. The encumbrance of profitless speculation is fairly thrown off; and the advance of medicine therefore, in strict investigation, is very rapid. The acquisitions that have been made in this century, and especially in the second quarter of it, are vastly greater than were ever made before in the same length of time.

The advance has been greater, however, in diagnosis than in therapeutics. The principal reason of this is obvious. It is very generally a more difficult achievement to adjust accurately our remedies to the varying phases of disease, than it is to make out a clear diagnosis. It is a much more compound intellectual process. There are more circumstances to be considered; and the relations of these circumstances are endlessly varied in the different cases. Therapeutics, therefore, really affords scope for higher mental powers, and especially for a greater compass of mind, than diagnosis. The general impression has been otherwise. This may be seen in the common remark, that, if the diagnosis be made out in any case, it is easy to treat it. The diagnosis, it is true, is the proper basis of treatment; but the same discrimination that has evolved it is to be exercised still more carefully and skilfully, in order to secure the adaptation of the curative means to the morbid condition, as it varies in its manifestations from day to day. For great suc-

cess, both in diagnosis and in therapeutics, there is required good sense in the highest meaning of that term, — a quality which is really both more rare and more valuable than great learning or brilliant acuteness of mind.

Another reason for the estimation which has been put upon diagnosis in comparison with therapeutics is to be found in the brilliant discoveries which have been made in the diagnosis of disease during the past half-century. To say nothing of others, Laennec has opened to us a vast and rich mine in diagnosis. Such discoveries could not but exert a wonderful influence upon medical men everywhere; and this influence has been enhanced by the marked general bent in the French medical mind towards the researches of diagnosis in preference to those of therapeutics.

Perhaps, considering the inherent difficulties attending therapeutical investigations, the advance has been proportionably as great in them as in diagnosis. That there really has been a great advance during the past quarter of a century is manifest from the facts which I have adduced in this Essay. And these facts show clearly what is the nature of this advance. It consists not so much in the discovery of new remedies, as in the discovery, one after another, of the circumstances that should govern us in the use of remedies which had already long been familiar to the profession. Though chemistry has made some almost new remedies out of old ones, by extracting their very essence, there have been really but few new remedies of any great value discovered. Our means of cure have been little added to in this respect; but they have been greatly increased by the discriminations which have been made in regard to their application. These discriminations have so varied the modes and degrees of their application as to add materially to the actual resources of the *Materia Medica*. While the gross amount used of most medicines is much lessened, what is used is generally applied with more clear and definite aim, and therefore accomplishes more good. At the same time,

less harm is done. In short, the use of medicines is not as large as it was, but is more definite and various, and therefore more available in meeting the individual variations of disease.

The future improvement in therapeutics is probably to be mostly in the same direction that it has been. Our resources are to be increased by multiplying their modes of application, more than by the discovery of any new resources. Some have indulged the hope that specifics of different kinds will be discovered for the cure of disease. Dr. Rush was wont to talk of the probability that some plant would be found that would cure consumption; and, even lately, Professor Alison, in his "History of Medicine," indulges in the anticipation that medicine will hereafter be much advanced by "the discovery of specifics, which may counteract the different diseased actions of which the body is susceptible, as effectually as the cinchona counteracts the intermittent fever; citric acid, the scurvy; or vaccination, the small-pox."¹ But, strictly speaking, there are no specific remedies, though there are a few that approach to this character. There may be some yet to be discovered: but we have no reason to expect this from our past experience; neither have we reason to anticipate very much in the discovery of new remedies of any kind. While the search for them should by no means be discontinued, it is not worth while to expend labor here which may be more profitably expended in farther observation of the relations to disease of the valuable remedies already discovered. The improvement in therapeutics resulting from such observation will probably be very much greater than it has yet been; for observation is all the time becoming more strict and accurate, and will increase greatly in these qualities if the profession come to be extensively

¹ How so acute a mind as Professor Alison's could think of the relation of vaccination to the small-pox as being similar to that of citric acid to scurvy, and that of cinchona to intermittent fever, I am at a loss to divine. The great fact that the vaccine disease will *prevent* the small-pox stands entirely alone. There is no other fact that has the remotest analogy to it.

governed in their medication by the principles that have been developed in this Essay.

This advance in therapeutics must be attended with a great diminution in the range of the *Materia Medica*. This must be largely sifted, that we may know what our reliable resources are. A useless polypharmacy has always encumbered it. Sydenham had some realization of this truth; for he speaks of the "immense stock of eminent medicines that we have long been pestered with," and seems to deprecate any addition to their number. Even as late as the beginning of the present century, the following strong language was used by Bichat in regard to the *Materia Medica*: "An incoherent assemblage of incoherent opinions, it is, perhaps of all the physiological sciences, that which best shows the caprice of the human mind. What do I say? It is not a science for a methodical mind: it is a shapeless assemblage of inaccurate ideas, of observations often puerile, of deceptive remedies, and of formulæ as fantastically conceived as they are tediously arranged." There has been a great improvement in the *Materia Medica* since Bichat's time, but more in the simplification of formulæ than in the diminution of the long array of medicines which the *Materia Medica* contains. Many of these ought to be excluded; and statements which are made in regard to others ought to be omitted, as not having yet been ascertained to be true by a careful observation. Such a sifting, as the strict observation contemplated in this Essay would give the *Materia Medica*, would probably show that many quite current ideas of the efficacy of medicines are mere vague fancies, and that there are comparatively few active remedies of real value; and, in relation to the multitude of new remedies which throng the pages of our medical journals with flaming representations of their efficacy, not one of them should be admitted among the real resources of our art till it has been fairly tested by experience.

I have spoken of observation as the means of effecting,

under the rules that I have laid down, the improvement in therapeutics contemplated in the proposition which is the subject of this Essay. And the inquiry arises here, By what method or methods of observation is this to be done? It is claimed by some, that the numerical method of Louis is the grand means of settling all questions of therapeutics, and indeed that nothing can properly be considered as definitely settled till it is verified by the tests of this method. If therapeutical investigations have the complex character that I have attributed to them, and if the circumstances which should regulate the application of curative means vary so much in different cases of the same disease, this method can throw but little light upon the action of remedies. It can be at best but an auxiliary in establishing some very general facts, and cannot aid us at all in adjusting the degrees in which remedies shall be applied in different cases, much less in arranging the combinations of remedies as the circumstances of each case demand. It fails in the very point in which it is claimed to be peculiarly serviceable; viz., in exactness. While it may prove some of the most general truths in regard to the relations of remedies to disease, it offers no tests for their exact and minute application under the various and fluctuating circumstances of individual cases. For example, it has proved the general truth that bleeding has commonly a curative influence upon pneumonia; but it teaches us nothing in regard to the character of the cases of this disease which call for this remedy, or the circumstances which should regulate its use; and even the general truth that it has proved in regard to this remedy was already abundantly proved by the common every-day observation of the profession. Indeed, I know of no truth proved by the numerical method, in regard to the application of remedies, which was not already established.

It is not my intention to go into an examination of the defects of the numerical method of observation; but I will merely remark, that, the greater is the number of remedies

that are applicable to any disease, the more signal is the failure of this method, from the great variation which is required in the different cases in the proportions of these remedies, and in the relative times of their use.

If therapeutics, then, were shut up to this method of observation alone, it would be made up of only a few bald generalities, and would not merit the name of a science. The industry of Louis, the founder of what is termed the Numerical School of Observation, is indeed to be admired; and his minute observation of disease is worthy of imitation: but his therapeutics, as seen in his great work on fever, is an entire failure, as might be expected from his adherence to so narrow a method of investigation. Any physician of ordinary attainments, using the common mode of observation under the guidance of plain good sense, has a much better knowledge of the proper application of remedies in cases as they arise, than Louis has with all his learning and acuteness. The truth of this remark, I think, would be manifest to any one who will candidly observe the manner in which he comments, in his work on fever, upon the four remedies, — bleeding,¹ tonics, blisters, and ice on the head, — on which

¹ I cannot see how any one, taking a common-sense view of the matter, can avoid coming to the conclusion, that, in almost all those cases reported by Louis in which bleeding was employed, it was not an appropriate remedy. In most of them, the disease was quite advanced before the bleeding was resorted to; and, in many of them, the symptoms were such as absolutely to forbid the practice; and, in some of them, there is decisive evidence in the record that it did harm. To sustain what I have said, I subjoin very brief notes of the cases in which bleeding was used:—

First case. On tenth day, venesection $\frac{3}{4}$ x. No improvement.

Second. On thirteenth day, forty leeches behind ears. No improvement. On eighteenth day, eighteen leeches to neck.

Fourth. After ailing three weeks, six days in bed; venesection $\frac{3}{4}$ viii.

Sixth. On seventeenth day, venesection [how much not said]; pulse 150; and face red and flushed before bleeding. After, pulse quicker and smaller, and respiration accelerated. Died at four, A.M., next day.

Seventh. On twenty-second day after first sick, twenty leeches to neck. No improvement. Prostration.

Eighth. Sick on 21st Oct.; “two venesections” on 26th; leeches to anus before. Venesection $\frac{3}{4}$ x. on 30th. On 31st, in morning, “face pale, covered with sweat, as if she were moribund; pulse very small and very feeble;” twelve leeches to ears.

he gives us his observations; and the attempt which he makes, in the conclusion of his work, to apply the numerical method of observation to the effects of these remedies, is a specimen of inconclusive and valueless reasonings, which has seldom had a parallel.

It is the minute and varied observation, of which the numerical method can take no account, that must be the basis of the advance in therapeutics contemplated in this Essay. It is this alone that can furnish the means of detecting all the limitations which the diversified circumstances of individual cases call for in the use of remedies; and it is the minute and complicated knowledge of the relations of remedies to disease, gained by this mode of observation, that

Ninth. On sixth day, venesection \S xv. No improvement. Next day, twenty leeches to ears. Died two days after.

Eleventh. On sixth day, venesection \S x. Died eighth day.

Fourteenth. On fourteenth day, twenty leeches.

Seventeenth. On ninth day, venesection \S xii. No call for it in the symptoms, and no improvement from it.

Eighteenth. On sixteenth day, venesection \S xvi.; forty leeches, next day, behind ears; venesection next day after, and thirty leeches behind the ears.

Twentieth. On fifth day, eighteen leeches to anus; leeches also two next days. On tenth day, twenty leeches to jaws; and eleventh, twenty to neck.

Twenty-first. On fifteenth day, venesection \S xii.; seventeenth, eighteen leeches to ears.

Twenty-second. On tenth day, venesection.

Twenty-eighth. On third day, venesection. Fourth day, venesection \S x. Sixth day, twelve leeches to ears.

Thirtieth. On twenty-fifth day, twenty-five leeches to abdomen; and twenty-sixth, fifteen leeches.

Thirty-first. On ninth day, twenty leeches to neck.

Thirty-third. On seventeenth day, venesection \S x.; next day, fifteen leeches to anus; and, three days after, twelve leeches to ears.

Thirty-sixth. On third day, venesection \S xii.; sixth, sixteen leeches to ears.

Thirty-seventh. On sixteenth day, fifteen leeches to anus; and same, two days after.

Forty-first. On twenty-fifth day, ten leeches, after perforation of intestine.

Forty-sixth. On eighth day, venesection \S viii.; leeches before.

Forty-eighth. On seventeenth day, venesection \S x.

Forty-ninth. On tenth day, venesection \S xii.

Fifty-third. On tenth day, venesection \S x.; same, next day.

Fifty-fourth. Had been sick about two weeks; venesection \S xii.

I could give notices of symptoms in more of the cases before and after the bleeding to verify what I have asserted; but it would make this note too long.

constitutes true skill in the art of medicine. The numerical method not only cannot impart this skill, but a strict adherence to this method is a bar to its attainment.

It has been sometimes asserted, that all conclusions arrived at in therapeutics are really numerical results, as if physicians were always practising a sort of mental arithmetic as they gather the results of their daily experience. Numerical estimates, it is claimed, are made continually, although there may be no consciousness of it; and the usefulness of a remedy in any disease is determined by such estimates. So far from this, there is always more of weighing than of numbering in common, every-day observation; for the relative value of circumstances in disease is very properly considered more important than the frequency of their occurrence. This is true even of symptoms, but it is more emphatically true of the effects of remedies; and, farther, it is by direct observation of the effects of remedies that the judicious practitioner judges of their applicability more, much more, than by any gross results that may be expressed by numerals. Indeed, these gross results, which are so much relied upon by Louis and his followers, are apt to be fallacious, unless extreme care be taken to have comparisons made between cases that are very much alike in their circumstances.

The skill or tact which is acquired by common observation, carefully and thoroughly pursued, is capable of being much improved by extensive experience. While it must be in the case of every one, for the most part, the result of his own experience, yet it may be greatly cultivated by a comparison of his experience with that of others. But this comparison can seldom be made; for there is really little experience properly detailed in the records of the profession. We have an abundance of recorded *results* of observations; but these are comparatively of little value when they are not accompanied to some considerable extent with the observations themselves, and especially when they are mingled in a

confused mass, as has been too often the case, with theoretical speculations.

The grand desideratum in therapeutics is *recorded minute impartial observation*. We need what Sydenham termed a natural history of diseases. We want cases of every kind of disease reported fully, with their treatment. With such records, we should have the data for making an extensive examination of the effects of remedies, just as we do in a limited manner in our own private practice. Louis has set a noble example in this respect. Let a multitude of observers make similar minute records of cases of disease as they arise, and a great advance will be made at once in therapeutics. I say cases as they arise; for it has been too much the fashion to report either extraordinary cases, from which little really can be learned, or cases selected from the mass because they resulted successfully under some particular mode of treatment.

If the actual experience of the profession could be extensively gathered in the manner indicated, it would manifestly tend to give great definiteness to our views of the action of remedial means, by the acquisition of an extensive knowledge of the circumstances which modify their action. It would tend, therefore, to banish that polypharmacy which is the legitimate result of indefinite views, and to secure the advance in therapeutics which we have been contemplating.

There is one interesting point upon which I will comment very briefly before bringing this Essay to a conclusion. It is the influence which a general resort to preventive means, or, in other words, an obedience of hygienic rules, would exert upon the character of disease, and therefore upon its treatment. This influence would be of such a nature as to favor materially the deliverance of practical medicine from its uncertainties. This will appear from the following considerations: The less complicated disease is, the more readily and clearly do we make out both the diagnosis and the curative indications. But what is the chief source of the com-

plications of disease? Evidently that series of morbid impressions to which the various organs are subjected year after year, producing successively points of disease that are ready to be waked up into activity at any time when any general disturbance is produced in the system. And these impressions are made mostly by causes which are to a great extent preventible. We see this especially exemplified in chronic diseases. These, which are always disposed to be complicated, are so generally accumulations of results from a continued action of the causes referred to, that it has been said, and with much truth, that chronic maladies are the natural fruits of our disregard of the laws of health, while acute diseases are the direct inflictions of Providence. But even the latter are, to a considerable extent, self-inflictions, though much less so than the former; for not only are the complications which so commonly aggravate acute diseases, and perhaps constitute their chief danger, the results, in part or wholly, of previous transgression of hygienic principles, but very often the direct causes of these diseases can be avoided.

Observe, now, what would be the whole scope of the effect which a proper attention to hygiene would have upon therapeutics. It would manifestly give to diseases generally a much more simple character than they now have. It would, therefore, simplify the relations of disease to curative means, and thus favor that simplicity of treatment which should be our great aim in all attempts for the improvement of our art; and, more than this, prevention, in delivering unavoidable disease to a considerable degree from its usual complications, would diminish its severity. While, therefore, the interference of art would be much less needed than now in aid of the efforts of nature, whenever it should be called for, it would be directed with a much more definite aim than is ordinarily possible with the complex character which the present common neglect of the laws of health so generally gives to disease. And the fact thus demonstrated,

that, besides all the direct good effects that come from an attention to the laws of health, there is an indirect influence upon therapeutics coinciding with our attempts for its improvement, is a consideration of no small importance.

In treating the proposition which is the subject of this Essay, I have deemed it to be but a small part of my duty to demonstrate its truth. I have looked far beyond this, and have endeavored to develop principles, the guidance of which, in the treatment of disease, would continually advance our art, relieve it of much of its uncertainty, and eventually place it upon a satisfactory basis. I have aimed to mark out the channels into which the energies of the profession must be directed to accomplish this purpose. I have considered it very important that physicians should have a right appreciation of the relative value of the various curative means which they employ, and especially that they should be aware of the necessity of great discrimination in the application of them to various and complicated and fluctuating states of disease. I have endeavored to bring out fairly the true dignity of the physician's office, showing the wide scope which he should give to his investigations and to his curative means, and the great value of those means which are neglected, often with ruinous and sometimes fatal effects, when the world's idea, that the physician is the mere doser of the body, rules in the sick-room. But, while I have shown that drugs are really subordinate means of cure, I have endeavored to guard very carefully against the danger of discarding them whenever there is good ground for expecting curative results from their use. I have endeavored to inculcate the nicest discrimination on this point, in opposition to the influence of a mere general vague idea, that drugs are to be as little used as possible, which produces in the minds of so many practitioners an indolent and indiscriminating reliance on nature's curative powers.

The improvements which I have noticed as having taken place during the past quarter of a century afford us glimpses

of the future of our art which are indeed bright with promise; for they were accomplished under great difficulties, such as would of course attend the beginning of a new movement. And when these shall be to a considerable extent removed; when the profession, as a whole, shall adopt right principles in the administration of remedies and in the observation of their effects; and when a general rational attention to public and private hygiene shall both greatly lessen the amount of disease, and render it more simple in its character, — therapeutics must be placed upon a basis, of which its present condition affords us no adequate conception. That the change will be a great one, we know; but we have no means of estimating its amount, or of giving any thing but faint indications of its character.

If the principles which I have developed in this Essay be correct, the field of investigation offered by therapeutics is a more inviting one than has commonly been supposed. The achievements that may be realized here may even vie with those brilliant results which have of late attended researches in diagnosis and pathological anatomy. Our art, it is true, will never cease to be a conjectural one; but it may be redeemed from the unnecessary confusion and uncertainty which false principles of observation have brought upon it, and be made vastly more definite in its aims than it is at present. To attain this, severe and patient labor will be required. Careful observations must be extensively collected by the profession, after the plan which I have indicated; and they must be investigated in the most searching manner.

This is the great work which is now demanded of the profession. The time has fully come for it to be done. The preparatory steps have been taken; the many changes that have occurred in medical practice during the past century or more have been manifestly preparing for it. Results have been accumulating which will favor its prosecution; and the recent improvements and discoveries in diagnosis

furnish a fitting basis for the full inauguration of this work. To such a work as this, the eminently practical character of the American mind is particularly suited. The French excel us in the researches of pathological anatomy, and perhaps in diagnosis; the English surpass us in the literature of medicine: but, in therapeutics, we are superior to both, especially to the French. In the grand movement which I have described as going on in practical medicine, the American school (if we can say, that, in our new and forming state, we have a school) has been thus far in the advance, whether we regard the general movement itself, or the particular improvements which have contributed to it. Let us, then, enter heartily upon this work, and do what we can to rid our art of its encumbrances and defects, and introduce fully the reign of a truly RATIONAL THERAPEUTICS.

